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THE EYE AND THE MICROSCOPE.

By Henry J. Slack, F.G.S., F.R.M.S.

THERE is a notion prevalent that using the microscope injures the eyesight, but this is really not the case; it is only the abuse of the instrument that has such an effect. It does not hurt the eye to look at anything which is plain and easy to see, and neither in too strong nor too feeble a light. The art, then, of using the microscope consists in exhibiting small objects so that they shall be seen as larger ones are without any instrumental aid. To do this the microscope must be a good one, and a few simple rules followed until practical skill is gained. Whether the microscope is a binocular or not, *both* eyes must be kept open. This is of absolute necessity if the sight is not to be damaged. Many persons find it difficult to look down a tube with one eye without shutting the other. A remedy for this, and one useful in other respects with a monocular instrument, is to cover a piece of cardboard nine inches long and five wide, with black cotton velvet, cut a hole in the middle, and insert the eye-piece through it into the tube of the microscope. With this screen in front, everybody finds it easy to keep *both* eyes open, and look at the object with either one, if they are both alike, or with the best if they differ. Persons whose eyes agree in focus, and otherwise correspond, derive the greatest benefit from binocular instruments, but the use of one eye does not fatigue it if the object is properly focussed and suitably illuminated. A common fault with beginners is to use too high power, with which it is impossible to see the object they want. For many most interesting sights of live objects a four-inch objective is extremely useful, and the highest power a beginner is likely to employ with advantage is a one-inch, with a couple of eye-pieces, the highest giving a linear magnification of 50 or 60, with the English length of tube. The proper use of this power should be mastered before more magnification is attempted.

It requires considerable practice to pay attention only to one part of an object that may be shown plainly, and take no notice of other parts that from any reason are not clear. With low powers, an object need not be quite flat for the whole to be fairly in focus at the same time; with higher powers, great flatness is indispensable, and an extremely slight irregularity only permits one portion at a time to be clearly seen. This state of things is very uncomfortable to a beginner, and the eye is sure to suffer from it. The size of an object that can be viewed as a whole with any power depends upon several conditions, which need not now be discussed, but the higher the magnification the less it is, and with an enlargement exceeding 200 linear it becomes exceedingly minute.

As soon as the student has attained to some dexterity in the use of the instrument, he should acquire the habit of paying exclusive attention to what he can see plainly, and take no notice whatever of things out of focus, or from any other cause not even focally visible. With some exceptions, it is most agreeable to the eye that an object should not occupy the whole field, but have a fair margin round it, which should not be over-flooded with light. Eyes differ very much in sensitiveness to light, and when, as in noticing the actions of live objects, prolonged attention is required, the intensity of the illumination should be nicely regulated to suit the individual case. Light passing through a piece of foreign post-paper, saturated with spermaceti, is often the most pleasant.

By attention to such directions as have been mentioned, there need be no fear of devoting a considerable time every day to microscopic investigation, and many observers could be mentioned who have done this for years without any detriment to their visual powers. On the other hand, those who torment their eyes in attempts to see the most difficult diatom markings, or the closest of Nobert's ruled lines, suffer from their folly, without any compensation in the shape of useful knowledge.

ABOUT FALLACIES.

By the Editor.

SEVERAL correspondents write about the question of luck as we considered it in No. 11, some asking whether the evidence does not show that some men really are exceptionally lucky (so that their luck in matters of pure chance may be relied on); others asking whether, if a coin had been tossed a great number of times with the same result (head or tail) in one set of trials, it would not be more likely to show the other side (tail or head) oftener in the next set of trials; while yet others consider that the ideas of men of science about fallacies generally are erroneous—that, in fact, the so-called fallacies are real truths.

Taking the last first, I may note that the rule of science in all those cases in which specific results are popularly supposed to follow from specified actions, or the like, is simply to inquire whether there can possibly be any relation of cause and effect in such cases. When a housemaid says, for instance, that putting the poker across a fire makes the fire burn up, the student of physical laws is able at once to see that the supposed influence is antecedently most improbable. Here in a grate are certain more or less combustible materials, and certain quantities of matter already burning; combustion is going on, though indifferently; the air is nourishing this slowly burning fire, but inefficiently; on the whole, it seems likely that the fire will go out. In what way shall I do any good if I stick a rod of iron from the fender across the top bar? I thus add a certain quantity of cold metal to the space across which the air has to come to the fire. Do I increase the draught? On

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the contrary, so far as I produce any effect at all on the draught, I must diminish it. For the draught depends in the main on the diminished density of the warmed air in the neighbourhood of the fire, and the cold metal must to some degree increase the density of this air by cooling it. The effect may be very slight, but, such as it is, it is unfavourable. But here is a correspondent who tells me that whether theoretically the poker should make the fire burn up or not, as a matter of fact it does. Repeatedly he has tried the experiment, and after exhausting in vain every art he possessed to make the fire burn up, he found the poker put across the top bar, immediately, or almost immediately, produce the desired result. Science is bound to listen to evidence of this kind, for science deals with phenomena, and even when phenomena seem to point to something which appears utterly incredible, science has to inquire into the matter. Well, in this case, what are the facts? Someone tells us that he has repeatedly tried in vain to make a fire burn up, but when he put the poker across it, the fire presently became clear and bright. Multitudes of contrary cases might no doubt be cited, but let us suppose that none could. Are we therefore to infer that in these cases the poker drew the fire up? A new law of nature would be indicated, if this were so; and a new law of nature is worth learning. But when due inquiry is made, it appears that there is no such law—as, unfortunately, we might have expected. Our correspondent, who found that when he put the poker across the fire it drew up, is unquestionably but an unskilful fireman. He puts on coals, and pokes and stirs the fire, unconscious of the fact that this is just the way to put a fire out. When the fire is all but hopelessly reduced by his unskilful measures, he puts the poker across the top bar. According to old-fashioned superstitions, he makes the sign of the cross across the fire-place, and the fire, in which until now there seemed to have been some evil spirit (that is what people mean when they say "the devil's in the fire"), is purified from the unclean presence and begins to burn up. That would have been the old-fashioned interpretation of the change; unfortunately, science takes another view of the matter. It sees reason to believe that the change took place simply because the disturbance to which the fire had before been exposed was bad for it. Putting the poker across the top bar meant letting the fire alone, and giving it a chance to burn up.

Singularly enough, I had occasion, when the last sentence was just finished, to leave my study. When I came back, an hour later, I found that my fire, which in the meantime must very nearly have gone out, had been recoaled—and the housemaid, or whoever had attended to it, had, after the fashion of her tribe, put the poker across the top bar. The fire was not burning very brightly—on the contrary, it seemed inclined to go out. Yet, rashly daring, I put the poker down—from scientific principles I object to seeing bright metal smoked and dulled—and went on with my work, intending, if the fire went out, to call someone in to light it again. However, it so chanced that after the poker was put down, the fire began to burn pretty brightly, and as I write there is every promise of a good fire. Am I to infer that taking the poker from across the top bar made the fire burn up? Of course, the real fact was, that when the fire seemed dull it was really making steady progress, and whether I had taken down the poker, or supplemented its salutary action by putting another poker across the top bar, would have made not one particle of difference.

That our domestic servants should consider the poker across the top bar a specific for making a dull fire burn up is very natural. Their manner of treating fires is unscientific in the extreme. A Cambridge Fellow, who knew very little about the fair sex, except what he might gather from the ways of "bed-makers" and his recollections, perhaps, of domestic servants at home, used to define woman as "an inferior animal, not understanding logic, and poking a fire from the top." Most servants do this. They also have two utterly erroneous ideas about making up a low fire—first, that the more fuel is put on the better; secondly, that after putting coal on it is desirable to stir the fire. As a matter of fact, when a fire is low, the addition of fuel will often put it out altogether, and the addition of much fuel is almost certain to do so; and in every case the time to stir the fire (when low) is before coals are put on, not after. Generally it is well, when a fire is low, to stir it deftly, so as to bring together the well-burning parts, and then to wait a little, till they begin to glow more brightly; then a few coals may be put on, and after awhile the fire may again be stirred and some more coals put on it. When a low fire has been unwisely treated by being coaled too freely, and the fresh fuel uselessly stirred, it is generally the case that the only chance for the fire is leaving it alone. Susan does this when she puts the poker across the top bar, and unconsciously she retains the old superstition that by thus making the sign of the cross over the fire, she sends away the evil beings, sprites, or whatever they may have been, which were extinguishing it.

That letting the sun shine on a fire puts it out is not, like the other (in its real origin, at any rate), a superstition, but simply an illusion. A correspondent writes that it is believed in by nine persons out of ten; but in this it is like all other wrong beliefs. Scientific methods of inquiry and reasoning are followed by fewer than ten in a hundred; and although now-a-days the views of science are accepted more widely than in olden times, this is simply because science has shown its power by material conquests.

I do not think that my friend Professor Tomlinson's experiments on the burning of candles in sunlight and in the dark would be regarded by all as decisively showing that sunlight does not interfere with combustion, though, rightly apprehended, they go near to prove this. But *d priori* considerations show conclusively that though by warming the air around a fire the sun's rays may, in some slight degree (after a considerable time), affect the progress of combustion, they cannot possibly put the fire out in the sense in which they are commonly supposed to do so; in fact, a fire would probably burn somewhat longer in a room well warmed by a summer sun than in a room from which the solar rays were excluded. (The difference would be very slight.)

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

TO-NIGHT we will avail ourselves of the Zodiacal Map, on p. 225, and examine some of the objects it contains which have not yet been described in these papers. Before commencing our stellar work, though, we will have a look at Mars, now travelling quickly away from us. He is apparently becoming rapidly smaller, as he is receding from the earth, and a good deal of the detail which would have been visible in the instrument we are employing at the end of last December, has now become imperceptible. With a power of 150 or upwards, though, the planet at moments of the best definition will be seen as in Fig. 17.

What is technically called the gibbous appearance of Mars will at once strike the observer's eye. In other

words, the outline of the planet will be seen to differ sensibly from that of a circle, a portion of such outline or "limb" being seemingly cut off by a curved line on the side opposite the Sun. This gives a somewhat humpbacked effect. (Lat. *Gibbus*, humped.) Hence the name.

Fig. 17.—Mars.

While in this region of the heavens, we may notice one or two objects in Taurus, which we omitted on p. 221, in the absence of means for their identification. They will be found in the Zodiacal Map, which we are now employing. The first is Piazzi V., 20 Tauri, which will be noticed in the second square to the left of, and a little higher than Aldebaran; it is marked 20^{p}. This will prove a very severe test of the excellence of the observer's instrument, and will require a fine night and the highest power at his disposal to be made out properly. 118 Tauri, again, is a beautiful small pair; it lies on the parallel of declination below β on the map. In noticing the nebula to the N.W. of ζ Tauri, we omitted to add that ζ itself is situated in a rather pretty and curious field.

Above Taurus lies the constellation Auriga, to the examination of which we proceed to devote ourselves. We will begin with 14, marked T (for triple) in the map; but we shall only be able to see it as a double star, the components being of a yellowish tint, and about half as far again apart as those of γ Arietis. A very pretty pair will be found in ω Aurigæ. This does not appear by name on the map, but is so close to the star at the top marked 4^{p} as to incline us to believe that they are intended for the same object. It is represented in Fig. 18.

Fig. 18.—ω Aurigæ.

θ Aurigæ, as a close and very unequal pair, will tax both the instrument and the eyesight of the observer to the uttermost to see it properly. When best seen it will appear as in Fig. 19.

Fig. 19.—θ Aurigæ.

5. Aurigæ (to the north of ω, or 4, just out of the map, on p. 225), is another star in which the diversity of size of the components and their proximity render its observation decidedly difficult. The student will see both these objects better with a high power than with a lower one. 26 (N.E. of β Tauri in the map) is a pretty star, from the contrasted colours of its components, and is very easy from their distance. The comparison is almost horizontally to the left of the larger star. $\Sigma\,\delta\,72$ is an equally easy pair. It will be found just to the left of the solstitial colure in the map. 225 P. v. Aurigæ, to the N.E. of 26, must be found by fishing, as it is invisible to the naked eye. When in the field of the telescope, however, it will be found to be a close and extremely pretty little pair.

We may conclude to-night's work by a glance at two or three of the most striking clusters of stars in the constellation under review. And first, M. 38 (north of ϕ Aurigæ) forms a beautiful field, the main cluster assuming a cruciform aspect. The telescope may be moved about in this neighbourhood, which is a rich one. M. 36 (nearly due E. of ϕ) is also very fine. M. 37 (N. of the double star 225, previously described) is a glorious field, even with such an instrument as that which we are employing. In regarding a nebula or cluster, no light should be suffered to enter the eye for some little time before it is applied to the telescope; and the observer should gaze steadily at such an object until the eye becomes accustomed to it, after which hitherto imperceptible detail will flash up. Another rich field will be found in $\frac{h}{3}$ VII. 33 (marked 33′ at the very top of the map).

Next week we will devote to Gemini and the constellations south of it.

Erratum.—Page 221, col. 2, line 22, ξ Tauri should be ζ Tauri.

THE ELECTRIC TELEGRAPH.

By W. Lynd.

The Battery.

BEFORE describing the instruments used by the Government and the Telegraph Companies for the transmission of news and private telegrams, a brief account of the generators of the electric current is necessary.

The simplest form of apparatus for producing electricity by chemical action is called the Voltaic cell, named in honour of the inventor, Professor Volta, a celebrated Italian philosopher. The cell is easily made, and the cost of the materials is trifling. Two strips of metal—one zinc and the other copper—a glass cup, nearly filled with water, to which a little dilute sulphuric acid is added, two bits of copper-wire, and the apparatus is complete. The wires must be soldered to the metallic plates. When the metals are immersed in the liquid and the extremities of the wires placed in contact, chemical action begins. The currents of electricity are produced at the expense of the zinc and the acid. The zinc is consumed and the water is decomposed. The zinc has a very strong affinity for oxygen. The chemical action going on in the cell when the metals are in contact is rendered visible to a certain degree. The liberated hydrogen may be seen collecting in bubbles on the copper plate. The zinc is acted upon by the acid. The oxygen, liberated upon the decomposition of the water, combines with the zinc, forming an oxide of zinc. The copper is not acted upon.

"The materials of an ordinary voltaic cell," Professor Thompson says, "may be regarded as the fuels of electric currents, just as coke and coal are the fuels of steam-power. Like those fuels, they represent a store of energy." In the voltaic cell, the flow of electricity continues so long as the wires are joined and the direction of the current is from the zinc plate through the acidulated water to the copper, and from the copper through the wire back to the zinc. A single cell such as I have here described would

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be of little or no use for telegraphic purposes, but a series of cells would produce a current sufficiently strong for the working of a needle or Morse instrument. The first battery that came under my notice was constructed as follows:—In a gutta-percha trough about 2½ ft. long, divided into compartments, were placed alternate plates of zinc and copper. The cells were filled with fine sand, over which dilute sulphuric acid was poured. This form was known as the Cruikshank or sand battery; it is now entirely superseded, for it was not constant, and required "refreshing" frequently, or the signals became so weak that it was a very difficult matter to read off a message on the double or single-needle instruments. The form of battery now adopted is called the two-fluid battery, and consists of a wooden trough, lined inside with a resinous composition, which prevents the action of the acid upon the wood. The trough is divided into ten or twelve water-tight compartments insulated from each other. In these cells stand porous earthenware pots, containing a solution of sulphate of copper (blue-stone), and surrounded by a semi-saturated solution of sulphate of zinc. Plates of zinc and copper are connected together by a band of copper, rivetted to each, and bent to allow the copper plate to be in one division and the zinc in the next. The coppers are immersed in the blue-stone solution with which the porous cells are charged, the zincs in the sulphate of zinc. The last copper plate is called the positive pole of the battery, and the terminal zinc the negative pole. When this battery is in action, the copper of the solution is precipitated on the copper plate. If all the solution were consumed, hydrogen would be deposited on the copper, and the current would lose its constancy. To maintain the solution in a saturated condition, and to prevent the accumulation of gas bubbles, crystals of sulphate of copper are placed in the porous cells. The zinc plate is consumed and the copper increased by the precipitation of the metal held in solution. The sulphuric acid produced by the decomposition of the sulphate permeates the porous cylinder, and tends to replace the acid used up by its action on the zinc. The quantity of sulphuric acid decomposed in the solution of copper is regular; the action of the acid on the zinc is regular also, and thus a constant flow of electricity is produced. Professor Thompson represents the chemical action as taking place in two stages

Zn	+	H_2SO_4	=	$ZnSO_4$	+	H_2
Zinc	and	Sulphuric acid	produce	Sulphate of zinc	and	Hydrogen.

and then—

H_2	+	$CuSO_4$	=	H_2SO_4	+	Cu
Hydrogen	and	Sulphate of Copper	produce	Sulphuric acid	and	Copper.

It will be seen that the zinc plate is destroyed and converted into a sulphate, and the copper plate actually gains by the action of the voltaic circuit. In the old sand battery, the current was enfeebled by the accumulation of hydrogen at the copper plate, and the zinc was consumed. In the battery I have just described, which is a modification of the cell contrived by the late Professor Daniell, the hydrogen bubbles are avoided and the current is constant, but the consumption of zinc goes on just the same. Now, the student will be able to comprehend one of the greatest discoveries of modern times—that the voltaic cell is *reversible*. "To every action there is an equal and contrary reaction." The application of Newton's words is not limited to mechanical reaction—it reaches down into other departments of science. In the science of electricity and magnetism, the same fundamental principle holds good.

Dr. Sylvanus Thompson, to whom we are indebted for the very best definition of the action of the voltaic battery, says:—"To separate an atom of zinc from one of oxygen requires energy to be expended. When thus separated, they have the chance of doing work in *re-uniting*, this work generally appearing in the form of heat. When a piece of coal is burned—that is to say, is permitted to unite chemically with oxygen—its store of energy runs down and manifests itself in the evolution of heat. A piece of coal represents a store of energy; so does a bag of hydrogen gas; so does a piece of zinc, for zinc can burn directly and give out heat, or may burn indirectly by being dissolved in sulphuric acid, also giving out heat. A Daniell's battery represents a store of energy. A pinch of gunpowder also represents a store of energy. The amount differs, it is true, and the rate at which some of these stores can be made available for use also differs widely in the different cases. An ounce of coal represents an amount of energy which, if entirely expended in doing work, would raise 695,000 pounds one foot high against the force of gravity, or would do 695,000 foot-pounds of work. In an ounce of gunpowder is stored about 10,000 foot-pounds of energy. An ounce of zinc represents a store of only 113,000 foot-pounds. An ounce of copper represents a store of about 69,000 foot pounds only. An ounce of hydrogen gas will yield, by combining with oxygen, 2,925,000 foot-pounds of work. Joule first showed us how to make use of facts like these in calculating by its mechanical value the electric power of voltaic cells. Let us apply these considerations to the storage of energy in any ordinary voltaic cell—say, for example, the Daniell's cell used in telegraphy. In this cell we have certain liquids containing zinc and copper chemically dissolved in sulphuric acid, and into these liquids dip a plate of zinc and a plate of copper. The zinc plate slowly dissolves away, and, at the same time, metallic copper is gradually separated out of the solution, there being about 1 1-20 oz. of zinc consumed for every ounce of copper deposited. Now, to separate an ounce of copper from its solution in sulphuric acid, requires 69,000 foot-pounds of energy to be spent upon it, and as 1 1-20 oz. of zinc represents a storage of 118,650 foot-pounds, the consumption of this weight of zinc is enough to provide the 69,000 foot-pounds needed to separate the copper and to leave a surplus of 49,650 foot-pounds. It is this surplus which goes to maintain electric currents in the circuit and do electric work. But, as we have remarked, the voltaic cell is reversible. If we could take such a cell and by means of some superior electromotive force drive electric currents back through the cell, the whole action will be reversed. Copper will be dissolved, and zinc will be deposited. The copper in dissolving will help the process by giving part of the necessary energy, and our currents will thus once more give us back pure zinc, and so separating out the zinc, we do work and actually store energy." To sum up, a telegraphic battery is a box divided into compartments containing plates of zinc and copper alternately arranged, and immersed in solutions of sulphate of copper and sulphate of zinc. The wires at the terminal plates are called electrodes. When the wires are joined together, the battery is said to be "in circuit." How the electric current is transmitted to distant places, and the instruments employed for the recording of signals will be explained in a subsequent article.

Preserving Fruit for the Winter.—Dry sand of all substances is found, from the experiment of P. Sarauer, to be the best in which to preserve fruit for the winter. The germs of mould attack the rough portion of fruit packed in paper, with great avidity, through the openings in the silk paper. If packed in straw, the least dampness of the straw imparts a musty flavour to the fruit. Sand has another advantage, which is that the damaged specimens do not infect their neighbours. Choose the most perfect fruit with the waxy covering perfect. Leaving the stalks on makes the fruit shrivel up quickly.—F.C.S.

THE GREAT PYRAMID.

By the Editor.

BEFORE considering the characteristics of the Great Gallery in detail, I must note one peculiarity which seems to me very significant.

Regarded as a sort of architectural transit instrument, the Great Gallery would, of course, have to be carried up to a certain height, and there open out on the level to which the Pyramid had then attained, the sides and top being carried up until the southernmost end of the Gallery was completed with a vertical section like that shown in Fig. 2 (further on). This would be the "object end" of the great observing tube. The observer might be anywhere along the tube, according to the position of the object whose transit was to be observed.

Now notice that the most important object of transit observations is to determine the time at which the objects observed cross the meridian. Either the observer has to determine at what time this happens, or, by noting when it happens, to ascertain the time; in one case, knowing the time, he learns the position of the celestial object in what is called right ascension (which may be called its position measured around the celestial sphere in the direction of its rotation) in the other, knowing the position of the object in right ascension, he learns the time. But whether the observer is doing one or the other of these things, he must have a time-indicator of some sort. Our modern astronomer has his clock, beating seconds with emphatic thuds, and he notes the particular thuds at or near which the star crosses the so-called wires in the field of view (really magnified spider lines). We may be tolerably certain that the observer in the Grand Gallery had no such horological instrument. But he *must* have had a time indicator of some sort (and a good one, we may notice in passing), or the care shown in the construction of the Gallery would have been in great part wasted.

Now, whence could his time-sounds have been conveyed to him but from the upper end of the Gallery? A time-measure of some sort — probably a clepsydra, or water-clock—must have been set there, and persons appointed to mark the passage of time in some way, and to note also the instants when the observer or observers in the Great Gallery signalled the beginning or end of transit across the Gallery's field of view. These time-indicating persons, with their instruments, would have occupied the space where now are the floors of the so-called Antechamber and King's Chamber—then, of course, not walled in (or the walls would have obstructed the view along the Gallery). These persons themselves would not obstruct the view,

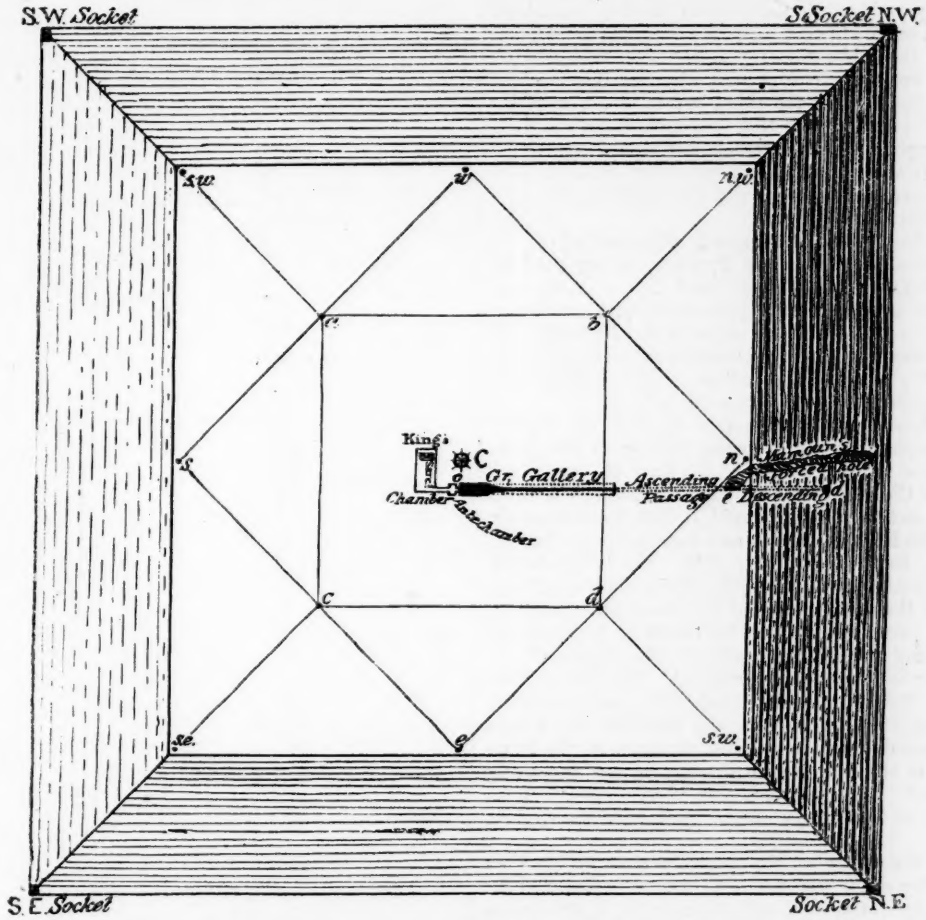

Fig. 1.—Showing the Flat Square Summit of the Pyramidal Observatory. [See next page.

unless they came too near the mouth of the Gallery. Or they might be close to the mouth of the Gallery at its sides, without obstructing the view.

But now, notice that if the place they thus occupied—the future King's Chamber (perhaps, as the region in or near which all the observations of the heavenly host in culmination had been made)—were in the centre of the square top of the Pyramid as thus far built, they would be very much in the way of other observers, who ought to be stationed at certain special points on this horizontal top, to observe certain important horizontal lines, viz, the lines directed to the cardinal points and to points mid-way between these. An observer who had this task assigned him should occupy the very centre of the square top of the, as yet, incomplete Pyramid, so that the middle point of each side would mark a cardinal point, while the angles of the square would mark the mid-cardinal points. Also this central point ought not only to command direction lines to the angles and bisections of the sides, but to be commanded, without obstruction, by direction lines from these points.

Thus the upper end of the Great Ascending Gallery should not be exactly at the centre, but somewhat either to the west or to the east of the centre of the great square summit of the incomplete Pyramid.

Let us see how this matter was actually arranged:—

Fig. 1 shows the incomplete Pyramid, as supposed to be viewed from above. The four sockets, *s.w.*, *n.w.*, *n.e.*, and *s.e.*, were supposed, until quite recently, to mark the exact position of the four base angles of the Pyramid. It turns out, however, that they are rather below the level of the real basal plane of the structure, which is, therefore, somewhat smaller than had been supposed.

Fig. 1 is, however, chiefly intended to show the nature of the square platform, which formed the top of the pyramidal frustum when the level of the floor of the gallery of the King's Chamber had just been reached. We have a horizontal section of the Pyramid, in fact, taken through the floor of the King's Chamber and Antechamber—that is, through SD, in the figure at p. 266. The bottle-shaped black space, near *O*, gives the section of the slanting gallery, beginning on the southern side at its widest part, reaching a narrower part somewhat to the north of *O*, and thereafter narrowing towards the north, till the section of the uppermost or narrowest part is reached. The dotted lines show where the Grand Gallery and the narrow ascending passage (ascending for one passing towards the King's Chamber) pass downwards into the structure of the Pyramid: at e is the place where descending and ascending passages meet. The position, also, of the entrance-hole, forced in by Al Mamoun, at about the level of the angle e, is indicated.

At *O* is the centre of the square surface, which then formed the top of the structure. If posts were placed at the angles *n.w.*, *s.w.*, *s.e.*, *s.w.*, and also at *n.*, *e.*, *s.*, and *w.*, an observer stationed at *O* would have the cardinal and the mid-cardinal points exactly indicated. Now the point *O* is about eight and a-half paces from the middle of the southern opening of the Grand Gallery; so that, if there were an assistant observer at *o*, he could communicate time signals readily both to the observers in the Gallery and to the observer at *O*. All such observations as the easting, southing, westing, and northing of heavenly bodies would belong to the observer at *O*, uprights of suitable height being erected at *n.*, *e.*, *s.*, and *w.* He could also observe when heavenly bodies passed the mid-cardinal directions, *n.w.*, *s.w.*, *s.e.*, and *s.w.* It will be noticed that if we suppose the Grand Gallery completed, which would carry it to a height of about 28 ft. above the level of the floor at *o*, the slant of the Gallery would yet be such that the observer at *O*, supposing him to observe by means of an instrument raised a few feet above the level of the floor, would be perfectly well able to look along the horizontal direction-line from *O* to *s.w.* (Most of his observations would, of course, be directed to points above the horizon.)

But I think if I were planning such observations on the square surface *e.*, *s.*, *w.*, *n.*, I should wish to have several observers at work in thus taking azimuths (directions referred to the cardinal points) and altitudes, just as several transit observers were manifestly provided for in the construction of the Grand Gallery.

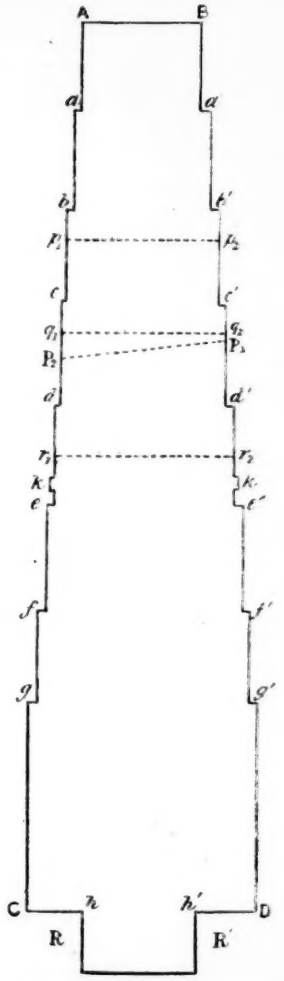

Fig. 2.—Vertical Section of Grand Gallery.

I should set an observer at *n.*, to observe in directions *n.-n.w.*, *n.-w.*, *n.-s.* (that is, *n.O.*), *n.-e.*, and *n-s.w.*; another at *w.*, another at *e.*, and another at *s.*, to observe in the corresponding directions belonging to their stations. Observers at *n.w.*, *s.w.*, *s.e.*, and *s.w.* could also do excellent work. In fact, they, between them, could take the horizontal cardinal and mid-cardinal directions better than the observer stationed at *O*, though his would be the best station for general work with the astrolabe.

Yet again, for observing heavenly bodies at considerable altitudes, stations nearer to the uprights at *s.w.*, *w.*, *n.w.*,

&c., would be useful. Where else could they be so well placed as at the points *a*, *b*, *c*, *d*, where the lines *w.s*, *w.n.*, *e.s.*, and *e.n.* intersect the diagonals of the square surface of the pyramidal structure? Note, also, that these observing stations would be at convenient distances from each other. The sides of this square surface would be roughly about 175 paces long, so that such a distance as

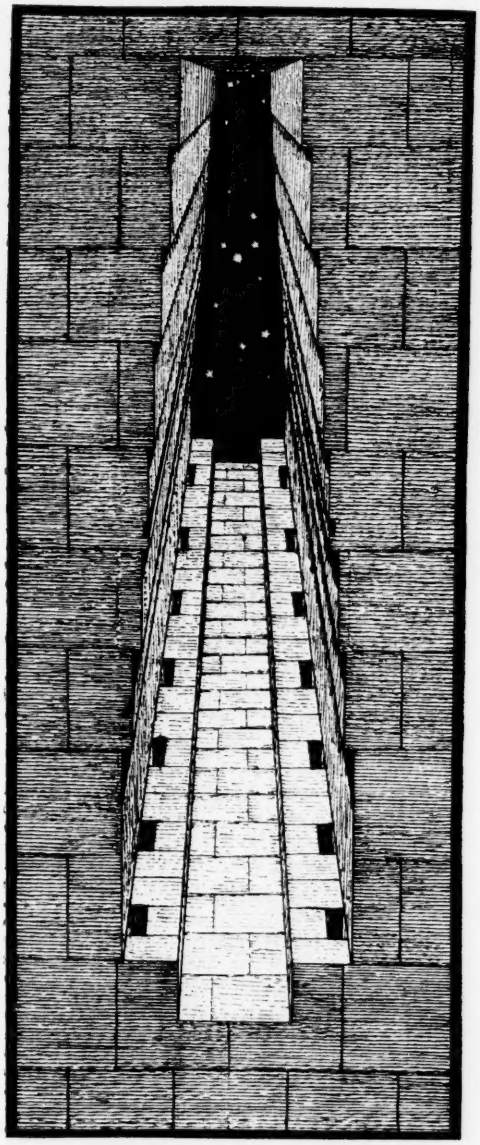

Fig. 3.—A Perspective View of the Upper Fourth of the Great Gallery.

a.w., or *a.O* would only be about 62 yards (the length of the Grand Gallery being about 52 yards).

Thus there would be thirteen observers of azimuthal directions and altitudes, whose work would be combined with that of at least seven transit observers along different parts of the length of the Great Gallery with its seven transit widths (as shown by its section, Fig. 2). Twenty observers in all, the transit workers, provided with the great fixed transit instruments in the Gallery itself, the others armed, perhaps, with astrolabes, armillary spheres for reference, direction tubes (or ring-carrying rods) would be able to make observations only inferior in accuracy to those made in our own time with telescopic adjuncts.

Fig. 3 is intended to show something of the structure of the interior of the Great Gallery. The stones outside are supposed to be seen in section, only one-fourth of the Gallery being given. For correct perspective, six or seven more layers of stone should have been shown below the lowest in the picture. But this would have given to the illustration an inconvenient shape. It will be seen that a section of the southern sky, very convenient for observation, would be seen from the interior of the Grand Gallery. The central vertical through this section would (as seen from the middle of any of the cross seats) be the true meridian. But the moment of transit might be equally well observed by taking the moments when a star was first seen (from the middle of a cross seat) on the eastern edge of the vertical sky space, and when the star disappeared: the instant midway between these would be the true time of transit. By combining the observations made by several "watchmen of the night," stationed in different parts of the Grand Gallery, a very close approximation to true sidereal time could be obtained.

I apprehend, however, that astronomers who had shown themselves so ingenious in other respects, would not have omitted to note the advantage of suitably-adjusted screens for special transit observations; and it seems to me likely that the long grooves shown in section at *k* and *k'*, Fig. 2, might have been used in connection with such a purpose, and not *merely* (though that was probably one of the objects they were intended to subserve) to carry a horizontal sliding cross-bar, by means of which the altitude of a celestial body at the moment of transit could be more readily determined. We must not forget that transit observers have to determine what is called the declination of a star (its distance from the equator), as well as what is called the right ascension, or distance measured parallel to the equator from a certain assigned point on that circle. For this purpose the horizontal lines *a a'*, *b b'*, &c., (Fig. 2) would be useful, but not sufficient. I incline to think that the method used to obtain accuracy in observations for determining declination involved a very practical use of the grooves *k k'*. Possibly a horizontal bar ran from *k* to *k'*, carrying vertical rods, across which, at suitable distances, horizontal lines were drawn (or, better still, horizontal rods could be slid to any required height). The horizontal bar could be slid to any convenient position, the vertical rods adjusted, and at the time of transit the horizontal rods could be shifted to such a height as just to touch a star as seen by an observer in the Gallery at the moment of mid-transit.

If a telescopist in our own time will try to plan out a method of determining the declinations and right ascensions of stars (say, for the purpose of forming a trustworthy star chart or catalogue), without using a telescope, by using such an observing place as the Great Gallery, he will see how much might be done, so far as equatorial and zodiacal stars were concerned; and they are altogether the most important, even now, and were still more so in the days when the stars in their courses were supposed to rule the fates of men and nations.

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THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

First Notice.

It is greatly to be regretted that this exhibition, like most others, is so slow in attaining to anything approaching what one may justly consider should be its proper dimensions. We are, of course, aware that there are many causes retarding the completion of the preparations, and that it is, generally speaking, to the interest of the exhibitor to get his show in a position fit for inspection as speedily as possible. It is, nevertheless, unreasonable to suppose we can endorse the unduly laudatory opinions expressed by the majority of unscientific critics. We shall see, as we go on, something of the great state of unpreparedness which pervades nearly every department, and although there is even now plenty of work for those who intend to thoroughly study the mines of information as they are laid open, it cannot fail, for some two or three weeks to come, to engender very great disappointment in the minds of those who can only pay one or two visits to Sydenham.

It is our intention this week to give simply a brief description of the general arrangements, and hereafter to describe in detail the various collections of apparatus, not forgetting to keep our readers posted up in the additions made to the Exhibition week by week.

The exhibits are divided into fourteen classes, some of which, however, may, so far as we are concerned, be regarded as sections of larger and more important classes. The class which commands the greatest amount of attention is that which embraces the various systems of electric lighting, while telegraphy appears to rank next in interest. There is one section of the exhibition, not favoured with a class to itself, which has great attractions for many of our readers, namely, that which comprises the various displays of apparatus for teaching the science of electricity. These will all be described in their turn. Commencing at the north end of the Palace, we enter first the machine-room of the Anglo-American Brush Electric Light Company. It is a spacious room, and has been floored with a layer of concrete, to give the necessary firm foundation for the machines. There is a goodly display of dynamo machines, all fixed in position, and most of them waiting for the arrival from the contractor of four out of the five engines required! The one engine is driving two dynamo machines, one of which supplies sixteen arc lamps, and the other a number of incandescent lamps of the Lane-Fox type.

There is also fitted up on one base a small steam-engine, working direct on to a small dynamo machine, capable of maintaining three arc lamps. The machine makes 1,100 revolutions per minute, and presents altogether a very compact and commendable appearance.

The arc lamps of this company are working well and steadily, and no doubt, when all of them are lighted, will receive universal admiration. The Lane-Fox incandescent lamps are tolerably good, but do not yet seem sufficiently steady, nor do they all appear to be equal in resistance, as some are much brighter than others. Leaving this company's exhibit, which is located in and about the Tropical Department we come into the nave, where, right away to the orchestra, there is a good display of arc lamps. The first are those of the Electric Light and Power Generator Company (Maxim and Weston systems). This light is very steady and very brilliant. It seems at present to leave nothing wanting in this direction. The company has also started a number of Maxim incandescent lamps, very tastily fitted into a kind of candelabra. They, however, are not uniform in brilliancy, for while some are equal to over twenty-five candles, others are little more than dull red.

Next is the display of the British Electric Light Company (Brockie system). These lights, nine in number, are very powerful; but it is to be feared they will never be quite free from momentary jerks, so to speak, which are, no doubt, due to the fact that perfectly homogeneous carbon-rods are unobtainable. Turning into the Egyptian Court, we find a sweetly-pretty collection of glass chandeliers and table-lamps of various designs (some of the latter several feet high), and fitted with the British Company's incandescent lamp. The glass is manufactured by Messrs. James Powell & Sons, Whitefriars Glass Works, and it is expected that they will all be ready for lighting in the course of a day or two. In the North Nave are also the stands of the Post Office, War Office, &c., including the almost empty cases of the School of Submarine Telegraphy. The Handel Orchestra is lighted by Messrs. Rowatt & Fyfe, with six Pilsen lamps, the same firm having ten Joel lamps in the Pompeian House. The latter at present appear very irregular. In the South Nave are four of Messrs. Strode & Co.'s arc lamps (Mackenzie system), which oscillate considerably. Messrs. Siemens & Co. and other houses should light the remainder of the nave, but they are all very backward. It is said that some firms fixed their plant on to the wooden floor. The immediate result can be conceived as easily as described. The concert-room is well lighted with 250 of Edison's incandescent lamps. They are extremely steady, and, owing to a peculiarity in the manufacture, are said to be very durable. By far the most attractive display is made by his exhibit in the entertainment-room, where he has 200 lamps arranged in all sorts of imaginable designs. In the centre of the room is a large brass chandelier, heavily laden with lamps placed in elegantly-tinted glass shades. The effect is grand in the extreme, and wins immediately the entire sympathies of all who enter the apartment. No amount of gas could give the brilliancy, for the quantity required would be much more than the air contained in such a room could support. To judge of the electric light, we must, of course, divest it of its gorgeous decorations, but even then we shall find it excellent.

Various exhibitors occupy the floor of the nave, while the costume-room is beautifully adorned by Messrs. Hammond & Co. (Brush System), who also light very effectively the corridor leading to the low level station. The gallery, which should be almost entirely occupied by exhibitors, is comparatively empty. This state of things is no doubt greatly due to the entire absence of electric light, and consequent nearly total darkness. On the ground-floor, Mr. Edison, The British Electric Light Company, and the Electric Light and Power Generator Company have their engines and dynamo machines, all well-fitted on solid foundations, but neither of them in a state of completeness.

Next week, some of the exhibits will be dealt with in detail, and the main principles of the more important apparatus explained.

NATURAL RUBBISH HEAPS.

By James Geikie, LL.D., F.R.S.

In a paper recently read before the Perthshire Society of Natural History, Dr. Geikie gave some account of the various accumulations of rock-*débris* which are now taking place in this country. He described the appearance presented by many of the mountain-tops and slopes in our hilly regions. The rocks were often more or less concealed below masses of coarse angular fragments of all shapes and sizes. The hill-tops frequently looked as if they had been subjected to the battering action of some mighty hammer, which had smashed and shattered the rocks to a considerable depth; so that, if we wished to get at the solid and undisturbed parent-mass, we should first have to clear away many feet, and even sometimes many yards, of more or less loose *débris*. The slopes of such *débris*-capped mountains were invariably clothed with long sloping taluses of similar fragments, which swept down at a high angle to the valleys, and at the base of these slopes large blocks and isolated masses of rock were of common occurrence. No one who should examine these phenomena could for a moment doubt that they owed their origin to the action of the atmospheric agents. Dr. Geikie then described, in a somewhat detailed manner, the mode in which the rocks were broken up by the disrupting force of ice. Water found its way into the crevices of the rocks, and, being frozen there, the joints were gradually widened by the expansion of the ice, again and again repeated. When this action took place on a flattish hill-top the rocks were simply disrupted, and the separate fragments pushed asunder. But upon the verge of precipices, and upon steep slopes, the disrupted fragments were shot downwards, as soon as thaw set in. There were other ways in which rock-*débris* or natural rubbish-heaps were formed. Strata were often undermined by the action of water, and large masses of rock, deprived of their support, tumbled down in ruins. This could be seen at the base of sea-cliffs, and along the margins of streams and rivers. Then, again, some kinds of rock which were more or less soluble in water were liable, under certain circumstances, to be disjointed and broken up. Limestone, for example, was dissolved by the action of acidulated water working its way downwards through the natural fissures of the rock. In process of time these fissures were widened by this solvent action, and converted into irregular channels and tunnels. This was the origin of most of our limestone caverns. Water continuing to percolate down into such caves, gradually loosened the limestone that formed the roof, and now and again large and small fragments of the rock, losing cohesion, fell to the ground. Another cause for the origin of rock-*débris* was to be found in the peculiar geological structure of certain masses of strata, which were so arranged as to render them liable to sudden and wholesale demolition. When a mountain was built up of a series of porous and non-porous strata, arranged in alternate layers, dipping into the valleys at such a low angle that the edges of the beds were exposed upon the mountain-slopes, such a mountain might at any moment be destroyed. Dr. Geikie then referred to several remarkable examples of such catastrophes. In the case of the Rossberg, in Switzerland, the destruction was due to the

fact that long-continued rains, soaking down through porous beds above, were arrested by beds of non-porous clay, which, however, became softened to such a degree that the mountain-mass of strata that rested upon them slid forward upon them, and rushed down into the valley. After describing yet other modes in which natural rubbish-heaps were formed, Dr. Geikie went on to remark that all the phenomena referred to were more or less exceptional, and that the agent which effected the greatest results was frost. Some of the other agents he had described could only work under certain geological conditions; — others, again, were somewhat limited in their action, and tended to remove the rubbish-heaps which they themselves had accumulated. But the action of frost in a country like ours was, he might say, general. It affected every part of the land, but of course the amount of work it performed was very variable. Its results were most conspicuous in mountain regions, where frosts were not only more frequent, more intense, and more prolonged, but where the physiographical conditions of the surface lent their aid in the most effective manner. The rock-*débris* gathered to the greatest thickness upon slopes at the base of a rocky precipice. This was natural, for the steep rocks above, shattered by frost, showered their *débris* downward. But on flat hill-tops the time must come when the formation of rock-*débris* must terminate. The rock would only be acted upon to as great a depth as the frost could penetrate. Some account of the frost-riven *débris* of other countries was then given, more especially of the Swiss Alps, and northern regions of Europe and North America. It was remarkable that many parts of our own country were covered with sheets of *débris* which had apparently long ago ceased to accumulate, and these sheets occurred not only upon comparatively low ground, but even in mountain regions. The angular fragments were grown over now with lichen and heath, and even with natural wood, and in every feature betrayed the marks of great antiquity. And not only so, but they occurred in positions to which loose blocks detached from the rocks at higher levels could not possibly have rolled. It was hard enough to account for the presence of such sheets of ancient angular *débris* in a country like Scotland, but it was more difficult still to explain the presence of similar sheets of angular *débris* at low levels in the South of England, in Northern France, in Southern Spain, and at many places upon the borders of the Mediterranean. After giving a description of the so-called "Head" of Devonshire, Cornwall, &c., and the similar accumulation upon the coast of Normandy, Dr. Geikie went on to give some account of the clay-with-flints of the Paris Basin and the great consolidated *débris*-heaps or *breccias* of Gibraltar. By means of sections across the Rock he showed the position of these *breccias*, and explained how they had been formed at two different periods, separated by a considerable time, during which the Rock of Gibraltar was submerged for some hundreds of feet. After remarking upon the fact that similar *breccias* occurred in Corsica, Sardinia, Malta, Italy, Cyprus, and other places, he proceeded to explain the mode in which they, the "Head" of Cornwall, the ancient *débris*-heap of Scotland, and similar formations elsewhere, had been accumulated. The angular fragments had been dislodged from the rock of which they once formed a part by the action of frost. But they could not have rolled to their present position upon the low grounds by the mere impetus acquired by them when they were disrupted from the rocks above. They would naturally come to rest upon the low grounds at the base of the cliffs, unless some other force than the mere impetus of their fall had been urging them forward. We now meet with them at distances of many hundred yards away from the foot of the cliffs and steeper slopes, and to have reached their present positions they have travelled over a surface-slope not greater in many cases than 5°, or even 3°. The *débris* speaks not only to the action of hard frost, but of heavy snows. It was the melting of the latter and the saturation of the *débris*-heaps which caused the rubbish to flow as it were outwards from the base of the cliff, and doubtless this action was still further favoured by the alternate freezing and thawing of the water-soaked masses. It might seem strange to speak of snows and hard frosts in the islands and along the borders of the Mediterranean, but the evidence of former colder conditions was not by any means restricted to ancient *débris*-heaps or *breccias*. In a few words Dr. Geikie then sketched the broad results which had been arrived at by glacialists as to the former extent of the European snow fields and glaciers during the Glacial Period, and he showed that these, taken in connection with the evidence furnished by organic remains, both animal and vegetable, abundantly confirmed the conclusions to which the phenomena of the ancient rubbish-heaps appeared to point. The climate of all Europe had been greatly affected; not only did an enormous ice-sheet, extending from Scandinavia and burying the British Isles, creep southward over the plains of Northern Germany, but all the mountain-tracts became centres of glaciation. The present glaciers of Switzerland were the degenerate successors of great icefields which now meet with their nearest analogues in the Arctic Regions. And many hilly districts in France, Spain, and Eastern and Southern Europe, which were now destitute of glaciers, were formerly the seats of extensive snow-fields and glaciers of no mean size. While in other places, such as the low grounds of Southern England and France, and hilly regions bordering on the Mediterranean, where the conditions were not favourable to the formation of glaciers, considerable snows fell, and hard frost ruptured and shattered the rocks. It was to this period of cold that most of those great accumulations of rock-*débris* belonged — those natural rubbish-heaps which had now ceased in many places to accumulate. They thus bore strong evidence to the former extent and intensity of ice-action during the Glacial Period.

DR. CARPENTER ON VACCINATION

At a monthly conference of the London Society for the Abolition of Compulsory Vaccination, held at the Steinway Hall (Dr. Andrew Clark in the chair), an address was given by Dr. W. B. Carpenter, C.B., on the increase of small-pox mortality in London during the year 1880. He pointed out the inadequacy of the objection that a system of compulsory vaccination outraged the rights of individuals, contending that in health, as in education, it was the paramount duty of the State to secure, as far as possible, the public advantage. The State, in his opinion, was morally bound to intervene in such a matter between the parent and the child, for the good both of the child and of society at large. He proposed to speak with special reference to the outbreak of small-pox in 1880, which, he understood, was specifically mentioned in the resolution that was to be moved in the House of Commons by Mr. P. A. Taylor. That outbreak, according to his view of the case, afforded grounds, not for the repeal of the Act, but rather for making its operation more complete and stringent. It was necessary first to consider the history of small-pox, with regard to which very important statistics existed in the bills of mortality for the last 200 years. In the case of other exanthemata – scarlatina, for instance—doubts might have been cast on the accuracy of the earlier figures; but small-pox had always been clearly recognised and distinguished from other diseases, and no such doubts could therefore be entertained. Now, from 1660 to 1678, the general mortality of the kingdom was 80,000 in every million of living persons, and the small-pox mortality was 4,170; in 1728-57 the general mortality was 52,000 per million, and the small-pox mortality 4,260; in 1771-80 the general mortality was 50,000, and the small-pox mortality 5,020—a slight increase, which was probably due, as Dr. Heberden said long ago, to inoculation. However, the average small-pox mortality in the period from 1660 to 1880 was about 4,000 per million. It was noticeable that at that time the disease periodically appeared in its worst form, and was the terror of all classes. Thus Louis XV. died deserted by all except Madame du Barry, and the priests who chanted mass in the Chapelle Ardente were said to have been "condemned" to do so. And in 1750 Horace Walpole wrote, "Lord Dalkeith is dead of the small-pox in three days." These, of course, were instances in which the disease appeared in its greatest intensity, and attacked the rich, who in these days would ordinarily have little to fear from it. He could scarcely suppose that an outbreak of small-pox—say, in Pimlico—would deter her Majesty from visiting Buckingham Palace. For the decade 1801-1810 the general mortality was 29,000 per million, and the small-pox mortality 2,040. In 1831-35 the general mortality was 32,000 and the small-pox mortality had fallen to 830. At that time he had himself seen as many as 100 cases of blindness from small-pox in unvaccinated persons, and it was probable that in the last century two-thirds of the patients at the eye hospitals were blind from the same cause, while the proportion now was only 5 per cent. In 1840 the Legislature provided the means of vaccination, and the result was that the mortality fell to 400 per million. Then came compulsory vaccination in 1853, and the small-pox mortality in the decade 1851-60 was only 278 per million. In 1861-70 the number was 276. He now came to the years 1871-80, which period was unquestionably exceptional. The mortality in these years among unvaccinated persons was so extraordinarily great, and the disease itself was so violent, as to suggest the notion that it might be indeed the Black Death of the Middle Ages. Yet, as far as he knew, no person who bore the evidences of vaccination had died of small-pox in the last year. In 1871 the disease was severe everywhere in Great Britain, but especially in Scotland, where compulsory vaccination had not been then adopted. Since that time, however, vaccination had been made compulsory in Scotland, where it was now enforced more effectually than in England, the result being that for the last five years there had not been twelve deaths a year in that country from small-pox.

XUM

London, on the other hand, thanks to the efforts of the society, there was an unvaccinated residuum which kept the disease alive. The epidemic had come to us from France, and had arisen there from the unsanitary condition of the French soldiers during the late war. Having regard to all the circumstances of the epidemic and from a study of epidemics in general, he had no hesitation in saying that the period 1871-80 was altogether exceptional, and that the rate of small-pox mortality during that decade afforded no basis for an argument against vaccination. He need only make one more observation. His opponents would doubtless urge that such places as Dewsbury, Leicester, and Keighley, where the anti-vaccinationists were strong, had had a comparative immunity from small-pox. But the truth was, that the disease had already died out in those towns, and that the mere disuse or neglect of vaccination did not reproduce it. As an illustration of the fact that no sanitation would suffice to exclude small-pox, the case of San Francisco might be cited. In the Chinese quarter of that city a smouldering fire of small-pox had existed for some time, but there had been no considerable outbreak since the autumn of the year 1879, when nearly 150 cases occurred in the best and richest parts of the city, in spite of the fact that, as the very low annual death-rate showed, the sanitation of the place was singularly good. Of the children, however, all of whom had been vaccinated—many from heifer lymph—only ten or twelve took the disease.

THE MOON'S BIRTH BY TIDAL EVOLUTION.

IN response to the wishes of many readers of Knowledge, we had intended to prepare for these pages a paper on the views to which Mr. G. Darwin has been led, and which Dr. Ball has eloquently expounded, respecting the birth of the moon by tidal evolution. It occurred to us, however, when our essay was nearly completed, that our readers might like to hear Dr. Ball himself on the subject; and we now have much pleasure in announcing that next week a paper from the pen of the Astronomer Royal for Ireland on the moon's birth by tidal evolution will appear in these pages; to be followed by another on the astronomical consequences of such evolution.

The Editor.

THE MENACING COMET.

READERS of Knowledge who have followed my remarks on the various predictions which have been made respecting the approaching end of the world, must have been inclined to exclaim:—"Is Saul also among the prophets?" when they heard that, as the *Spectator* tells us, I had definitely indicated the year 1897 as one in which the world would, in all probability, come to an end. I have carefully read over the essay in my recently-issued "Familiar Science Essays," to see whether it should suggest these startling anticipations; and I find nothing there which does not seem in perfect accordance with observed facts and scientific deductions therefrom. All that is there said I certainly adhere to still. How far it can be regarded as threatening the end of the world in 1897, I shall give the readers of Knowledge an opportunity of inferring next week, when a short article on the comet which is thus—thinks the *Spectator*—to bring the world to catastrophic end will appear, illustrated by a picture of that menacing object. Possibly after reading that article, those who have urged me to reconsider my verdict may find that some chance is still left for our good old earth.

R. A. Proctor.

Errata.—Lines 9, 11, 12, and 13, p. 270, for "wood" read "woad." Line 37, "nitril" for "nitrite." Lines 39 and 49, for "ortho-nitro phenyl-glyoxalic" read "ortho-nitro-phenyl-glyoxylic."

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

OUR LETTERS, QUERIES, AND REPLIES.

[268]—Letters, queries, and replies reach us in such numbers, that not only are we unable to find room for a third of them, but they involve a tax on our time seriously interrupting the progress of more important matters. We are obliged, therefore, to adopt the following rules:—

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

We beg that correspondents will consider how the matter stands. It is essential that each number should contain a certain portion of original matter, illustrations, notes on science and art, short extracts from home and foreign journals, mathematics, and so forth. We cannot yet enlarge Knowledge more than we have already done; to compress our correspondence into the space left open for it, by abridging, extracting, and putting the matter so arranged into proper form for the printers, would require either the whole time of the editor, or the assistance of a staff of sub-editors. Our correspondents must, therefore, do the work of abridgment and arrangement themselves; at any rate, they must not be annoyed if, failing this, their communications are wasted.

To ninety-hundredths of our readers no apology for this notice need be given. On the contrary, some explanation may be due to them for the way in which correspondence has threatened to interfere with the proper work of Knowledge.

The Editor.

FLESH FOOD.

[269]—I trust you will allow me to point out in your columns the extreme unfairness of the comparison you suggest between vegetarians and flesh-eaters. You ask vegetarians to produce a list of famous men of their persuasion equal to that which can be made out by their opponents. Now this would be an excellent and perfectly fair test, if exactly half the civilised world had always been vegetarians; but as matters actually stand, it is grossly unfair. You might as well ask the Quakers to make out such a list, or the Albinos, and the test would be just as valuable. The list of famous men, if it is to be of any use, must be made out with due regard to the proportion between vegetarian and carnivorous candidates for fame, and from this test the vegetarians have no cause to shrink.

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Your instance of Newton is not altogether a good one, as he lived on vegetarian principles while he wrote the "Principia," from the desire to keep his mental faculties unobscured.

J. L. JOYNES.

[Our correspondent does not seem to notice that we were saying jestingly what he himself gravely (and of course correctly) points out.—ED.]

THE MOON AND THE WEATHER.—INTRA-MERCURIAL PLANET.—THE ICE-AGE IN BRITAIN.—ACTION OF THUNDERSTORMS.

[270]—Mr. Bulley (p. 247) seems to me to be one of that numerous body who mistake assertion for proof. I will deal very shortly first with his science, and next with his facts. Now, *imprimis*, he repeats a very old fallacy indeed, when he says that "it must be clear to every tyro in natural science, that if it be rational, and in accordance with the verification of science, to assert that the moon's influence acts upon the waters of the ocean, it cannot be foolish and irrational to hold that the same influence affects the waters of the clouds and the air in which they float—both ponderable bodies, equally subject to the laws of gravitation." Just so. The late Professor Daniel conducted an elaborate series of barometrical experiments on the summit of Box Hill, in Surrey, and showed conclusively that there is actually a semi-diurnal *tide* in the atmosphere. But what then? If the gravitational action of the moon on our atmosphere affected terrestrial meteorology, the weather ought to change twice a day! Does it? We are told, though, that atmospheric changes occur when the moon either crosses the equator or attains her greatest north or south declination. Let us try this theory for the last three months. Writing with my own daily Meteorological Register and the *Nautical Almanac* open before me, I will see how far Mr. Bulley's theory holds for this part of England. On Nov. 22, 1881, at two a.m., the moon was on the equator. The cold cloud and damp of the previous day continued; from nine a.m. to nine p.m. the barometer rose exactly 0·002 inch; and, I may add, precisely the same weather (with, however, a rise in temperature) prevailed for about a week. The moon was again on the equator on the 29th, and, once more, nothing happened. Nov. 8 (when she attained her greatest north declination) was foggy. I am ignorant whether, according to Seleno-meteorology, great north declination of the moon should bring fog. On Nov. 22, when she attained her greatest south declination, the wretched wet weather from which we suffered, both before and after that date, persisted without change; and one of several gales which visited us towards the end of the month, blew in some parts of the country.

In December the moon reached her greatest declination north at 1 a.m. on the 6th; and here again the wet weather which had persisted (and subsequently persisted) from the beginning of the month, underwent no alteration whatever. On the 13th, however, when the moon was on the equator at noon, it actually ceased raining, or practically so, for twenty-four hours. The moon reached the most southerly point of her orbit at 10 a.m. on December 20; but the wet which distinguished the month continued. At 1.30 in the afternoon of December 26, the moon was once more on the equator, and here again nothing whatever was noticeable save the great height of the barometer, though this endured from the 23rd to the 29th. On the 18th there was a tremendous gale, and an almost equally heavy one on the 20th. On the occasion of the former and fiercer storm of the two, the moon was at some considerable distance from her "stitial colure." If we turn now to 1882, the moon attained her greatest north declination at 8 a.m. on the 2nd, and again we had a heavy gale with rain. At 8 p.m., on the 9th, she was in the equator, and again it blew, as it had done on the 5th, 6th, 7th, and 8th. At 9 p.m., on the 16th, the moon reached her greatest south declination, but not the slightest change took place in the fog and calm, which begun on the 11th, and lasted eleven or twelve days. Lastly, our satellite was on the equator once more at 10 o'clock last night (22nd), the high barometer of the last twelve days persisting, and one or two temporary peeps of clear sky being vouchsafed; otherwise, no change whatever occurred. Now, what are we to say to all this? Here we find all sorts of weather occurring when the moon has great north declination, great south declination, and no declination at all. Mr. Bulley makes certain assertions, but assertion is not proof; and the crucial test of any such theory as his is for every meteorological observer to institute such a comparison as I have attempted here. His concluding paragraph about "the chemical rays" of bodies whose diameters subtend angles of 40″, 30″, 16″, and so on, scarcely merits any serious reference.

Mr. Jones (query 188, p. 255) may possibly be thinking of the utterance of Le Verrier, in the *Comptes Rendus*, for Dec. 21, 1874, thus oddly translated in the R.A.S. *Monthly Notices*, vol. xxxv., p. 155:—"There exists in the neighbourhood of Mercury, doubtless between the planet and the sun, a matter (sic) as yet unknown. Does it consist in one or more planets or in more minute asteroids, or even in cosmic dust? The theory tells us nothing on this point. On numerous occasions trustworthy observers have declared that they have witnessed the passage of a small planet over the sun, but nothing has been established on the subject ['on n'est parvenu à rien coordonner à ce sujet']."

Referring "Arachnida" (query 192, p. 255) to that most interesting book, Geikie's "Great Ice Age," for full details, I may say here, that the stones frozen into icebergs and glaciers are polished and striated in a most striking and unmistakable way, as they are pushed over the rocks by the motion of the masses of ice in which they are imbedded, and that such polished and striated stones are found over nearly every part of the United Kingdom. Moreover, where the beds belonging to their geological horizon are fossiliferous, their contained fauna is arctic.

I think that what "F.A.S." (query 193, p. 255) apparently regards as a fact is at least questionable. The great heat and approximate saturation of the atmosphere which generally precede summer thunderstorms supply the most favourable possible conditions for acetous fermentation; and both beer and milk not infrequently turn sour during very hot weather without any thunder at all. Acetous fermentation is, though, a process of oxygenation, and the abundance of free ozone in the air during a thunderstorm may proximately or remotely affect liquid organic compounds, although, if this were the case, it is hard to see why no acidification takes place during winter thunderstorms. I am inclined myself to regard the belief that lightning turns beer sour because it does not know how to conduct itself as a popular delusion.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

INTELLIGENCE IN ANIMALS.

[271]—Some years ago, a small terrier belonging to a neighbour, having shown a propensity to come and chase my cats about my orchard, I repelled his visits with a stick, in flinging which I calculated the distance he would have run by the time that the missile reached the ground. I thought he had learned the lesson that I intended to teach him. Instead of which, he had thought of a dodge. I saw him one evening approaching the house, when my weapon flew as before. To my surprise, instead of bolting right away, he rushed towards me several yards, and then turned sharply round, and was off. The stick, therefore, instead of dropping objectionably near to his heels, flew high over his head, as he had calculated that it would. Thus was I outwitted by a dog—to my great delight.

Bards are often set down as but a nonsensical, visionary sort of people; but the following lines are among the many that might be quoted from the writings of poets, to show that they often display evidence of the possession of more common sense than can be boasted of by some of those who think themselves to be scientific philosophers:—

"Then vainly the philosopher avers
That reason guides our deeds, and instinct theirs.
How can we justly different causes frame,
When the effects entirely are the same?
Instinct and reason how can we divide?
'Tis the fool's ignorance and pedant's pride."—*Prior.*

As an illustration of a dog's capacity to distinguish between the characters of man and woman, I may mention that once, when a farm-labourer was telling me that a certain big, vagabond dog made himself a nuisance in the neighbourhood by entering the cottagers' rooms as he pleased, on my asking why they did not order him out again, he replied, "Ay, but he won't go out *for a woman*." Which feminine trait gave him this boldness to please himself in the matter?

F. RAM.

INFLUENCE OF SEX UPON MIND.

IV.—DAILY EXPERIENCE.

[272]—The common opinion founded on observation, that woman's reasoning and reflective powers do not equal man's, is not confined to physiologists deciding from head-forms and organic structure; but is held by the vast majority without theory, judging solely from experience and practical knowledge. Whately* defines woman as "a creature incapable of the exercise of reason, and that pokes the fire from the top." It is a colloquial axiom that you cannot agree with a woman. I have heard many lady-lecturers; not one argued consecutively; from beginning to end it was declamation! They

* Whately, was it? Like most other stories, it has been told of others; and in my college term it was told of one of our mos profoundly logical dons.—ED.

began by begging the question, assuming the very position they should have tried to prove; jumped to a conclusion logically unattainable; praised all who agreed with them as *friends*, and savagely scolded all opponents as *enemies!* "Female disputants, learned or unlearned, seldom know how to reason. They assert and declaim, employ wit, eloquence, and sophistry to confute, persuade, or abash adversaries; but distinct reasoning they neither use nor comprehend" (Miss Edgeworth's "Letters for Literary Ladies"). Woman's gift is certainly not argument; as well expect her to chop wood as logic. (I have seen American women chop wood!) In generalisation, arrangement, and concentration of ideas, she cannot compete with man. "They never see, whether for good or bad, more than one side of any question, and always the one which first presents itself" ("Oliver Twist").

Inability to argue is no mark of woman's absolute inferiority or non-development. Sexual equality advocates admit woman's lack of reasoning faculty compared with man. But (deceived by their hypothesis) they declare the power exists *latent*, and could be developed to the same extent as in man by a masculine education. They might as well argue that the power to equal man in size and strength exists latent in woman. On their assumption, inability to argue is a defect in woman. I maintain it is the result of natural organisation; therefore, no more a defect than woman's lack of manly size, strength, shape, beard, and complexion. Plato wished to submit the sexes to the same physical training, but even *he* declared woman in every respect weaker than man. Her mind corresponds with her body. Some men grow impatient with female relatives for inability to argue. Some despise the sex for this peculiarity. The "strong-minded" libel the "weak-minded" sisterhood as poor, stunted, distorted, mentally-arrested creatures! (*Victoria Magazine*, May, 1870.) All are wrong who adduce absence of reasoning power as a defect in woman. The obvious error is to gauge woman's mind by a masculine standard, and to expect the astounding absurdity that woman can, and should, possess all man's mental qualities in addition to her own! Wiseacres blurt out that woman is stupid, that her education has been neglected, because she cannot write like Locke, Bacon, Newton, Shakespeare, and Milton. Inability to argue—which would be a defect in man—is a characteristic and valuable *quality* in woman!

It is a most irrational conclusion that woman is mentally undeveloped, and claims our pity, and masculine, mental, and physical culture, to enable her to hold her own. Does man's pre-eminence in reasoning power give him an unfair advantage over woman? It would do so, if woman were—what sexual equality advocates misrepresent her—man's disappointed rival, an undeveloped younger brother, with a long lee-way to make up. "Woman is not undeveloped man, but diverse" (Tennyson). Two sexes constitute humanity. To tell women to copy man is a gross insult. She was no more intended to argue with, than to fight with, man. Very serious consequences would result from the sexes having minds constituted alike. Suppose that woman could generalise like man, could ascend to principles, could think as profoundly, and reason as correctly; and that man had woman's intuitive powers, and capacity for details. Woman would then become man's rival, instead of his help-meet. Each sex being able to dispense with the other's mental qualities, man and woman would live in perpetual discord. But at present, in spite of woman's alleged mental defects, harmony reigns between the sexes. There is constant reciprocal need of the male and female mind supplementing one another. All tends to mutual inter-dependence and happiness. Each sex, in turn, follows the other's impulse, listens to the other's advice; each influences in his or her respective province; each obeys, and both rule.

J. McGrigor Allan.

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[273]—At the beginning of his letter of the 13th, Mr. McGrigor Allan makes an assertion which, although true in the abstract, cannot logically be advanced as an argument for the mental inferiority of women. It may be that in the past men have practically monopolised the control of human thought and human institutions; but this circumstance is no more a voucher for their intellectual superiority than the defeat of the Romans was proof of the mental supremacy of the Huns. It has been largely a question of physical force, the assertion of which is proportionate in despotism to the ignorance or degradation of the male community.

What authorities can Mr. Allan cite for his statement that savage life shows the nearest approach to physical equality of the sexes? The hardships to which savage women are accustomed from their infancy are certainly such as could not safely be imposed upon civilised females. But are we to suppose that savage life has not increased male robustness in an equal proportion?

Mr. Allan incidentally remarks that "among savages woman is a slave." This is a fatal admission; for savages do but give physical scope to the spirit of overbearing which animates scoffers at women. Name a nation where women are debarred from social influence, and you have named one which is proportionately backward in liberty and knowledge. But, to be consistent, Mr. Allan should agitate against the part which women already take in state affairs. He cannot surely resign a large share in such an important function as the education of future generations into the hands of those who, he declares, do not possess "sufficient development of the abstract principles of justice, morality, truth, &c., to hold society together for one week!"

It is a remarkable fact that Mr. Allan's school, although it has ever impeded and opposed, as it still does impede and oppose, the higher education of women, always seeks to make an argument of their small achievements in invention and philosophy.

Mr. Allan hath it that "the eternal subordination of woman is conclusively exemplified in her exaggerated admiration for the male prerogatives—strength and intellect." Your readers must judge as to the soundness of the proposition that admiration—whether exaggerated or not—for strength and intellect is evidence of inferiority; but I have always heard that a profound appreciation of talent was the special characteristic of the world's greatest men.

For the rest, Mr. Allan's letter makes a series of pompous and sweeping assertions, supported by an extract from a novel, and so spiced with illiberal flippancy as to contrast strangely with his complaint of female injustice.

E. Burke.

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[274]—As "Only a Woman" considers the philosophy of Shakespeare conclusive on the subject of "women possessing justice," may I call her attention to a few things that the subtle understanding of the immortal bard has given forth to the world. He says, "Frailty, thy name is woman!" "Be it lying, note it, the woman's; flattering hers, deceiving hers;" "Women are frail as the glasses where they view themselves;" "Even to vice they are not constant." About their logic, he says, "I have no other but a woman's reason." Satirical view of their constancy: "Constant you are, but yet a woman; and for secresy, no lady closer;" "How hard it is for women to keep counsel."

So it will appear that Shakespeare does not represent woman as "infallibly faithful;" and I think it is only just to bring forward his opinions, since they have been courted. Some of his female characters are actuated by the most selfish and vicious motives that can possibly be conceived.

R. C. Fraser.

[To say that Shakespeare makes certain of his characters express these views, would be nearer the mark. What Shakespeare himself thought cannot be judged in this way.—Ed.]

ASBESTOS PAINT AND THE SAFETY-LAMP.

[275]—Upon reading the very interesting description in Knowledge of the successful experiments recently carried out at the Crystal Palace with the asbestos paint, I was led to infer that another important application of it—namely, to the wire-gauze of the ordinary safety-lamp—might be adopted. For, if this gauze were so protected, it would not, I conceive, even under the most unfavourable circumstances, be raised *above* incandescence, and, therefore, could never, while entire, give rise to an explosion, Sir Humphrey Davy, in his treatise on the safety-lamp, having declared "that even red-hot gauze of the proper degree of fineness will abstract sufficient heat from the flame of carburetted hydrogen to extinguish it." In fact, on account of the very low conducting property of asbestos, and the consequent difficulty of raising it to a high temperature, I ventured to propose, some years since, the substitution of an asbestos gauze or netting for the iron-gauze cylinder of the "Davy," but do not know whether the suggestion was ever experimentally realised.

Should you deem this brief communication worthy a place in your valuable pages, I shall feel gratified.—Yours, &c.,

W. H. O.

VEGETARIANISM.

[276]—Permit me a few lines of comment on part of a letter relating to the above subject, published in your last number, and signed "A Fellow of the Royal Astronomical Society."

If stories be really valuable in controversy, I can produce scores, the moral of which is exactly contrary to that cited by your correspondent. But my experience of "stories" is this, that they are seldom related with scientific exactness, and that minute investigation generally reveals some detail which has been either wilfully or ignorantly suppressed in narration, and which invalidates the whole point it is sought to establish. Personal observation of facts in one's own immediate sphere, constitute, in my opinion, the most valuable kind of statistics. Some five years ago I had very severe symptoms of tubercular phthisis, a disease hereditary in my family. The physicians whom I consulted recommended me cod-liver oil,

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raw meat, and what is commonly called "good" living. They were, however, of opinion that these means would but ameliorate my condition temporarily, my fate being sealed. As I was a vegetarian, and had begun to study medicine, I did not put into practice the advice given me. Instead of the raw meat, I took cold porridge made of oatmeal and milk, maccaroni, and other farinaceous foods, with as much fruit as I could get. I used hygienic means also, with the details of which it is unnecessary to trouble you. But I took no drugs, and no fish-oil. Instead of dying, I recovered my health, and shortly afterwards returned to my hospital course in Paris. Four years afterwards, I took my degree, and it is now my custom to recommend to my patients the dietary which saved me from death. I have found several of my patients greatly improved in health by following my example, and I have never found one the worse for it.

As I am "fanatic" enough to be quite sure I am right, I can afford to meet objections to my mode of life with equanimity, knowing that they proceed only from insufficient consideration or pardonable ignorance of the true bearings of the question.—Yours, &c. ANNA KINGSFORD, M.D.

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[277]—Having inserted a letter (207, p. 251) in which an altogether wrong construction is put upon the principles of vegetarianism, I hope you will allow the other side a little space for reply. Vegetarians are not such fools as to say that those living on a moderate amount of animal food, and temperate in other respects, cannot have health, though we think that a total exclusion of flesh, with the substitution of suitable vegetable products, would give yet better health and greater longevity. We can point to hundreds of cases where weakness and constant sickness has, after the adoption of a proper vegetable diet, given place to comparative health and perfect freedom from sickness. "A Fellow of the Royal Astronomical Society" gives an instance of greatly-increased mortality and illness resulting to some prisoners who were fed "principally on white and grey peas and lentils with bread." This is not vegetarianism: these results are only to be expected from such a concentrated highly nitrogenous diet, particularly as they were prisoners and, I presume, not doing any hard work. Many, upon making up their minds to try vegetarianism, think they must eat twice as much (many old vegetarians, however, only have two meals a day), and that, too, of the richest and most concentrated food—peas, beans, &c. As a natural consequence, they find themselves gradually getting worse. They and their friends, therefore, decry the system as a delusion. I could name some of the greatest thinkers and hardest workers who have been vegetarians.

A FELLOW OF THE CHEMICAL SOCIETY.

ANIMAL LANGUAGE.

[278]—From what I have read and seen, I have always taken it for granted that animals have languages of their own. But Arachnida evidently thinks that proofs are wanting to show that such is really the case.

Thoreau says that the language of birds may even, to a certain extent, be understood by man. His passage of the birds trying to fill up the hole in the roof (the chimney, wasn't it?) is very amusing, and at the same time full of interest to the reader.

Sir Samuel Baker, speaking of the monkeys on the banks of the Nile, says, that by watching them constantly he, by degrees, began to understand the meaning of some of their noises and signs, *i.e.*, their language. I forget if he uses the word *language*, but I think we may take it for granted that *he* thinks the animal kingdom has its various languages, just the same as we have.

If Arachnida has a cat who has a kitten, he will, by watching and listening carefully, find out that the old cat has a language by which it speaks to its kitten. For instance, I have noticed that by making a certain noise the mother will *call* its kitten to her, whereas another time she will, perhaps, make a different noise, which will be answered by the kitten, when the mother will run to the kitten, who will stay where she is—instead of running to its mother as in the former case. I have noticed this over and over again, as we have had a good many kittens, and I take an interest in watching them, and have tried them, as they have grown up, with *looking-glasses*, &c., to see the difference in their *mental* powers; some, being very sharp, finding out the deception very quickly, while others, as with us, have been stupids. But this is a digression.

I think there ought to be no doubt about the fact of an animal having a language of its own.

Your able article on "The Intelligence of Animals" has very nearly brought me round to believe in the *abstract* power of an animal to reason.—Yours &c., F.C.S.

Queries.

[228]—MICROPHONE.—Will you or any of your readers kindly give me full particulars as to the construction of a dry pile suitable for using with the microphone? Have looked up Guthrie's "Magnetism and Electricity" and Ganot's "Physics," the only works I have at hand on the subject, but although they give the material and arrangement of such a pile, they do not give the size and number of the elements.—G. B.

[229]—HAIR.—Is it possible for a person's hair to turn white *instantaneously* from fear, or other causes? If so, how is it accounted for? Have any well-authenticated cases been known?—PERCY B. DODD.

[230]—TELESCOPE.—Will "A. P. M." (letter 238) kindly inform what kind of black paint he used to darken the cartridge paper he used to make the tube of his Astro-telescope? Whether it was oil paint; and if so, of what kind? And also if he used any kind of dryer in the paint, as turpentine? And also if he can inform me whether or not he put into the tube diaphragms or not?—DALETH.

[231]—CHEMICAL PROBLEM.—Would any reader of KNOWLEDGE inform me how the co-efficients of any chemical equation may be calculated, the full equation being given, except the co-efficients? Thus given—

$$Cu + HNO_3 = Cu(NO_3)_2 + H_2O + NO$$

to find the numbers, or co-efficients, 3.8.3.4.2.:—

$$3\,Cu + 8HNO_3 = 3\,Cu(NO_3)_2 + 4\,H_2O + 2\,NO.$$

A method appeared in the *Chemical News* some years ago, which I cannot get now.—G. H. MAPLETON.

[232]—CHEMIST.—Would it be possible for a young man, nineteen years of age, having no knowledge of it, to be able, after severe study, but at not too great an expense of money, to pass the several examinations needed to set-up as a chemist? What books would you advise to commence, and also to proceed with? The probable expense of passing? The probable time?—W. A. FYSON.

[233]—BIOLOGICAL.—Will Dr. Wilson, or any of your contributors, kindly state—(1) Whether the difference between the highest ape and the lowest man is any greater than between the lowest man and the highest man? (2) Of what organ among the lower animals is the thyroid gland believed to be a rudiment? I cannot clearly understand from Hæckel's description in "Evolution of Man." (3) Whether any instances are on record of children being born with the caudal vertebræ projecting so as to form a rudimentary tail? Also whether it is true that a race of men exists with the projecting vertebræ? If so, who are they, and where is their country? (4) What is the brain capacity, general form and appearance, and relativity to man of the Neanderthal skull? What geological formation was it found in? No works to which I have access throw much light on this subject, and a short article would be very useful.—JOHN HAMSON.

[234]—DAISIES.—Mr. Grant Allen will serve some hundreds of us lawners if he will tell us how to free our lawns from daisy beauties.—A LADY FLORIST.

[235]—ANIMALS' FOOD.—Can Prof. A. Wilson give me any fact or theory showing that all animals were at one time herbiverous? I am often inclined to think so, and that carnivorism is an acquirement.—T. R. ALLINSON.

[236]—THE POLAR SUN.—At the North Pole, how many minutes does the sun's disc occupy in rising? and along how many degrees of the horizon does the sun move in the same time?—R. W. I.

[237]—PHYSIOGRAPHY.—Can you tell me of any text-book of physiography suitable for the advanced stage of the science and art examinations? Huxley's by no means covers the whole ground indicated in the syllabus.—GRADATIM.

[238]—ELECTRIC.—What is the "co-efficient of induction" between two inductors—a term which Maxwell frequently uses but nowhere defines? Also, is there any electric force outside a galvanic circuit?—O. A. BRIDGE.

[239]—SPANISH BOTANY.—Can any of your readers tell me if there is any work on the botany of Spain similar to "Hooker's Students' Flora of the British Isles," written in either English, French, Italian, or Spanish?—T. HUCKLEBRIDGE.

[240]—MEDIÆVAL.—What was the "luntes yelde," or "luntis yeld," or "luntys gylde," or "luntis yield" collected by churchwardens from the parishioners in 1505?—PHILO.

[241]—"FAURE ACCUMULATOR."—(1) Are all the tongues to point one way? (2) Which are joined together? (3) How is one cell connected with another? (4) Is there any limit to the amount a cell can hold; if so, what? (5) Ought the cells to be open or closed; if the latter, how? (6) Would five small Bunsen's cells be enough to charge it? (7) Should the lead be lifted out of the acid while not in action? (8) Is the force of the battery equal to that of the cells by which it is charged?—ECCENTRIC-CHUCK.

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[242]—Prose Composition.—Will you kindly do me the favour of saying whose work you consider to be the best on English prose composition?—Stanislas.

[243]—Air-Pump.—(1.) I have a small air-pump with one barrel. I have tried to filter mercury through a little cane filter sold for the purpose, but without success. Would some reader of Knowledge kindly give me a reason for this? Does the experiment require a stronger pump? (2). Would some reader mention a book containing experiments with the air-pump?—X. X.

[244]—Lectures.—I wish to give a few scientific lectures to young people, and should be grateful if some one would suggest good subjects, telling me where to get information, and how to make simple instruments for experiments? Where can I get information for a lecture on "The Atmosphere," and what experiments could be made by one who is unable to buy costly apparatus?—X. X.

[245]—Moon's Brightness.—The sun's light striking moon more obliquely, when near new moon, one would expect brightness per unit of surface to be greater. Is this so?—C. T. B. [Our correspondent surely means less, not greater. It is theoretically less, and actually less.—Ed.]

[246]—Barometer.—What is the mean height to mercury barometer at the equator?—C. T. B. [29·853 inches; though I should, for my own part, be disposed to doubt about the last decimal figure.—Ed.]

[247]—Warmth at Night.—I am a slight sufferer from chronic asthma, accompanied, chiefly at night, with a few attacks of coughing; and friends have urged the use of gas, conveyed through tubing to a stove filled with "asbestos," to warm the apartment. Is it conducive, or not, to one's health to maintain the warm temperature all night?—J. M. J.

Replies to Queries.

" [152]—"The Art of Electro-Metallurgy," by G. Gore, LL.D., F.R.S., (Longmans) 1877. I succeed perfectly where C. T. B. fails. —W. Van Eys.

[179]—Faure Accumulators.—Other things being equal, the power of Faure cells are to one another as the superficial area of the plates; but by using several plates in one cell, and coupling the alternate plates together, there is some gain, inasmuch as you use both sides of the plates. It appears to me that it is erroneous to call the Faure cell an accumulator. The Faure cell is, when charged, a mere Voltaic cell; and the action that goes on in the cell, when discharging, is similar in its nature to that which takes place in an ordinary Voltaic cell. The same remark applies to all secondary batteries.—H. B. T. Strangways.

[188]—Longevity of the Tortoise.—The late Professor G. Pryme, of Cambridge, mentions having paid a visit at the palace of the Bishop of (I think) Peterborough, and being there introduced to a tortoise, said, by tradition, to be then 200 years old. Going to the same place some ten or twenty years later, he found that the patriarch was defunct. The passage occurs in a very interesting biography of the Professor, written by his daughter.—E. D. G.

[191]—Animal Language.—Probably animals possess means by which they express their feelings one to another, be those means movement, looks, or even articulation. But considering their social state, we have no reason to expect that the Almighty endowed them with powers of speech as the human race. The members of the brute creation, living lives almost independent of each other, while the very existence of the human race depends on the mutual help, and therefore on the mutual intercourse of its members, what language may exist between animals is of a kind vastly inferior to that between man and man. But one finds, on studying the animal world, that this language is more perfect in the higher than the lower orders, some of the latter being entirely mute; the neighing of horses and ponies, the cawing of rooks, the greeting of dogs, and the language of birds being illustrative of the former, and the lowing of kine and the silence of fishes of the latter. Apart from the language which Æsop and La Fontaine put in the mouths of animals, and the whistle which some other individual placed in the mouth or the shell of the "Oyster of Drury-lane," we may conclude that some inferior means of intercourse exist, and that to a greater extent of perfection in the higher than the lower orders.—Herbert R. Weller.

[192]—Ice Age in Britain.—That an ice age or glacial epoch has existed in Great Britain, we may infer from the facts: that in some parts the eminences are all precipitous towards the west, the result of some powerful agent wearing away that side; that in the intervening valleys, boulder clay (a blue clay, in which rounded and waterworn pebbles are imbedded) is found; that deep furrows across the country, and striations (scratches) on the rocks, are observable, the striations and furrows being all parallel to themselves; that all the eminences are rounded at their summits, the result of some powerful agent passing over them.—Herbert R. Weller.

[192]—Ice Age in Britain.—In answer to "Arachnida," p. 255, as to what proofs exist, showing that there ever existed an ice age in Britain, reference to any geological work of any pretensions would have showed him the country fairly teems with proofs, from the Thames to Cape Wrath. The three great witnesses of glacial work are (1) the transportation of erratic blocks; (2) the smoothing and scratching of the valleys through which the glaciers travelled; and (3) the presence of arctic fossils in glacial deposits. As typical instances, Staffordshire contains erratic blocks transported from the Cambrian group; rocks from the Grampians are found 60 and 100 miles south of those mountains; while in Wicklow an immense block of granite is perched up 650 ft. above sea-level, and ten miles from the nearest granite. Valley scratchings are typically exhibited in the neighbourhood of Snowdon. Among the arctic fossils are *Trophon Clathratum*, *Astarte borealis*, and *Peeten Islandicus*.—W. G. Rolfe.

[195]—Quicksilver non-Poisonous.—It may interest "F.C.S." to know that in former times liquid mercury was given in large quantities, even pounds, for obstruction of the bowels. As to its efficacy, I cannot speak. The fact is, that ordinary liquid mercury passes through the digestive tract without being absorbed, and, therefore, without producing any effect upon the system. If, however, it be reduced to a state of very fine division (as is really the case in grey powder, blue pill, and some other preparations of mercury), it is absorbed, probably because its fine state of division enables it to be easily converted into oxide.—F. W. G.—[In parts of Ireland in former years a dose of small shot used to be given for a similar purpose.—Ed.]

[195]—Quicksilver non-Poisonous.—For the sake of F.C.S., I quote the following words from "Taylor on Poisons":—"Although liquid mercury is not in itself poisonous, it is liable to be converted into poisonous compounds in the body," p. 360, 3rd Edition.—Robert Macpherson.

[197]—Messrs. J. & E. Hall's last designs are by far the best in the market, not only for efficiency, but on account of their simplicity and the small space they occupy.—Gay.

[200]—Leases.—"James Gregg" should consult an actuary, or study Sir Isaac Newton's "Tables on the Value of Leases," or the modern "Tables" of Inwood, or both of these authorities.

[200]—Leases.—To determine the value of a lease, even when the conditions *are* specified, is not always an easy matter. To the following seemingly simple problem, I have received a score of answers, all varying. I would be glad of an authoritative solution. A. pays for a fourteen years' lease, £1,050; the rent for the first ten years is to be £250, and £300 for the last four. At the end of ten years he agrees to sell the lease to B. at a proportionate price to what it cost him. What is the amount B. must pay A.?—W. Cahill.

[204]—Minima of Algol.—G. M. T.

	H. M.		H. M.		H. M.
Feb. 16,	4 33 a.m.	March 11,	3 4 a.m.	April 3,	1 13 a.m.
,, 19,	1 22 a.m.	,, 13,	11 53 p.m.	,, 5,	10 24 p.m.
,, 21,	10 10 p.m.	,, 16,	8 42 p.m.	,, 23,	3 17 a.m.
,, 24,	6 59 p.m.			,, 25,	0 6 a.m.
				,, 28,	8 55 p.m.

Professor Pickering's observations, however, at Cambridge, U.S., show that in 1880 the true time of minimum preceded that of the ephemeris by about 37 minutes. "L." would do well, therefore, to look out for the minima about three-quarters of an hour before the times given above. Algol begins to diminish about 4 hours before the actual minimum, and does not regain its full lustre till 5½ hours after the epoch of faintest light. Curiously enough, Algol has a companion at about 82″ distance, which is also variable, but in some long period. It would take up too much space to answer "L.'s" second query adequately. An east wind has sometimes a very curious effect, rendering the discs of bright stars triangular, or causing them to be apparently accompanied by a faint, close companion. I found the latter effect very markedly visible on two occasions in the early part of 1874. Webb had noticed the same thing. Perhaps the Editor would say if he has ever heard of the "triangular" effect having been noticed in America.—H. Sadler.

[206]—Opium.—In chronic lead poisoning, the constipation, which is usually very obstinate, is due to a tonic, *e.g.*, a continued contraction of the muscular coats of the small intestine. If opium were administered in such a case, it would act as a purgative, by overcoming this spasm.—Robert Macpherson.

[207]—Heat.—The hand can be put into molten iron. The perspiration induced by fear provides a cushion of vapour, as in spheroidal state. If tried too often the experimenter has got

burnt. Wetting with ammonia acts better. A ring worn has caused burning.—C. T. B.

[210]—HOMER.—Both the "Iliad" and the "Odyssey" are published in prose in Bohn's Classical Library, price 5s. each.—C. J. C.

[213]—ORGANIC COMPOUNDS.—"Siquis" should read Berthelot's "Chimie Organique fondée sur la Synthèse," Paris, 1860. See also "Indigo," &c., in last number of KNOWLEDGE. Alizarin (chief colouring matter of madder) got by Græbe and Liebermann from anthracene. This can be built up from its elements. If "S." will write, will gladly show how.—C. T. B.

[218]—Tennyson.—"In Memoriam," poem 87 :—

"And over those ethereal eyes
The bar of Michael Angelo,"

Whose brow was straight and prominent, the sign of intellectual power. *Ibid*, poem 91 :—

"The sea-blue bird of March,"

The kingfisher, which like other birds, puts on its best plumage in early spring.—*Vide* "Key" to "In Memoriam," by Alfred Gatty, D.D.—P.B.F.

[218]—"The bar of Michael Angelo" refers to the peculiar contraction of the forehead, forming a wrinkle, seen in the old portraits of Michael Angelo. Arthur Henry Hallam, referred to in the quotation, had this same mark on his brow.—JOHN CRAIG, JUN.

[218]—"SEA-BLUE BIRD OF MARCH."—Kingfisher (*C. Ravenna*).

"These fields made golden with the flower of March,
The throstle singing in the feathered larch.
* * * * *
And down the river, like a flame of blue,
Keen as an arrow flies the water-king." —OUDEIS.

[219]—THE ATOMIC THEORY.—Daubeny's "Introduction to the Atomic Theory," Oxford, 1850, is the best English work. There is no theory to explain insolubility; bodies of similar chemical constitution dissolve each other.—C. T. B.

[220]—CHEMICAL ANALYSIS.—The most complete book on Qualitative Analysis is by C. R. Fresenius, translated into English, and published by Churchill, at 12s. 6d. The methods, however, are longer, and take up more time than most students can spare, but they are the best and most trustworthy. A very good book on the same subject is "Practical Chemistry," by Jones (Macmillan, 2s. 6d.). In "Quantitative Analysis" (inorganic), by far the best and most complete is Fresenius's (Churchill, 15s.); the large number of methods and quantity of matter is, however, confusing to the beginner, unless under the guidance of a teacher. On the same subject, Thorpe's is very good (Longmans, 4s. 6d.). If agricultural chemistry is wanted, there is Church's Laboratory Guide (Van Voorst, 7s. 6d.); the first part is qualitative, the second quantitative. The standard work on Volumetric Analysis is Sutton's (Churchill, 15s.). On Commercial Organic Analysis we have Allen's, of which only the first vol. is published (Churchill, about 15s.). "Practical Chemistry," by Blyth, is an excellent work on foods, drinks, and toxicology (Griffin, about 12s.). Wanklyn has written separate small treatises on the analysis of water, milk, tea, coffee, and cocoa (Trübner); "Water Analysis"—Frankland (Van Voorst); "Portable Water" — Ekin (Churchill); "Butter" — Hehner (Churchill, 3s. 6d.); "Commercial Handbook of Chemical Analysis," by Normandy, is in dictionary form (Lockwood); "Select Methods of Analysis, by Crookes (Longmans, 12s. 6d.); "On Microscopic Analysis of Foods," Hassell's is the best (about 24s.). There are a number of important articles on food analysis in the *Analyst*, a monthly magazine (Baillière), which every food analyst should see. Do not confine your attention to any one book on water analysis; it is necessary to read Wanklyn's, but analysts do not follow it throughout. There are so many works on qualitative analysis that it is difficult to say what are the best.—A FELLOW OF THE CHEMICAL SOCIETY.

[229]—MORTALITY FROM CANCER.—H. A. Everest will find in Haviland's work on "The Geographical Distribution of Heart Disease, Cancer, and Phthisis, in England and Wales," illustrated by coloured maps, the facts proving that cancer is influenced by locality. The above work was published in folio in 1875. It is now out of print, but can be obtained at second-hand booksellers, and seen at the Library of the British Museum.

[Letter 220]—THE HEALTH OF NAVVIES.—In No. 12 of KNOWLEDGE, I find betel-nut chewing given as a specific against fevers. The Javanese and Sundanese are inveterate betel chewers, and yet they have been dying literally by thousands during the past few years from fever. When I left Samarang, Java, in September last, the natives were dying by hundreds of fever. My experience, extending over nearly four years in various parts of Asia, has been, that the natives who universally chew betel (with lime, and the leaf of the sirih-pepper-plant), are much more easily affected by fever than are Europeans.—EDWIN SACHS.

Answers to Correspondents.

*** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule* 3, *free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

S. S. G. See Dr. Ball's paper in our next.—C. ICHABOD W. Kindly put query in concise form.—WALTER W. It would save much trouble if you would either put your queries in form, with heading, &c., or head your letter properly. Does moonlight really make the planets and their satellites more distinct? That three-inch *is* a splendid telescope. Having seen Rigel, as described in "Half-hours with Telescope," know that it can be so seen. Possibly the objects you refer to were observed under unfavourable conditions. A new edition of Webb's "Celestial Objects for Common Telescopes" has been published lately.—JOSEPH DAVIDSON. There is Rodwell's "Science Dictionary," Moxon's, price 10s. 6d., I believe.—E. W. Propose to re-write the articles on the "Differential Calculus," with sundry improvements which have occurred to me.—ZARES. When did we say that a tangentially (horizontally) moving mass has no energy? If a fly pushed against a mass a ton in weight so suspended as to be perfectly free to move in the direction in which the fly pushed, he would communicate to the mass just so much momentum as corresponded with the force wherewith he had pushed it. Or conceive half a ton of matter connected with another half ton by a strong but weightless cord passing without friction over a pulley, and let a fly light on one of them. Let his weight be one-ten-millionth of a ton. Then the weight on which he lighted would immediately begin to descend, the accelerating force being one-ten-millionth part of gravity. In 10,000,000 seconds, or 115 days, 17 hours, 6⅔ minutes, the velocity communicated would be 32 feet per second—that is, the same velocity which gravity communicates to a falling body in one second. It would take rather more than 3½ days to communicate a velocity of one foot per second. As to your offer of £5 prize to determine what force would overcome the inertia of a pound of iron, we are much obliged to you, but must decline. We repeat your other query; we might safely offer a prize ourselves to any correspondent who can understand what you mean. You require "the summation of the infinite number of infinitesimal differences between 0 and 1, of which 0·5 is the intermediary or ½." The sum of the differences would simply be 1. You do not mean that, however, for your original query asked for the sum of the series 0 to 1, when the number of terms is infinite. *That* sum, if there were such a series, would be infinite. You say I "dare not say" two ships unequal in mass, moving with equal velocity, could both be stopped with the same resistance: I dare say not.—F. BLAKE. Your method already given. But for the present we have done with magic squares.—GRANVILLE SHARP. You are more sharp than generous or reasonable. Before you spoke of my "foolish utterances" in the *Spectator*, you should have made sure they were mine. As it chances, they are not, nor do they in the slightest degree represent my views. Seeing how rashly you rush to conclusions, I am almost pained to think you ever thought well of my work. How sad to think that your past liking may have been as ill-founded as your present disfavour!—H. A. B. Statement insufficiently exact; you say nothing of size of building and height; you do not define your "very near."—G. E. R. Science knows of no way of inflicting pain on persons at a distance, "notwithstanding such obstacles as closed doors and windows, without any visible means." The stories about such action at a distance are generally thought to illustrate the influence of imagination.—F. E. B. The writer of "Brain Troubles" has some singular experiences to relate respecting music in the ears; the phenomenon is unquestionably subjective.—O. DAWSON. Pray define *inferiority* (in the Man *versus* Woman question), *man* (in the Descent debate), *true* (in the Phrenology discussion), and *justice*—in your own way. If you had any conception of the value of time, you would understand what our definition of *injustice* might be. We have no time for hair-splitting, and it is unjust to expect us to follow you in yours. We could find space for a short letter giving your definitions, and commenting (at reasonable length only) upon what you regard as the inexactness of others.—CLERICUS. We receive letters from America which are printed by some new form of type-writer, probably the one to which you refer. Perhaps some of our readers can give us information about the new, cheap

(beginners') type-writer recently brought out in America.—E. S. Dr. Ball (whom we have invited to write for us on the subject, and who has kindly done so) is quite right; the energy of a body in raising tidal waves varies directly as the mass, and inversely as the cube of the distance; that is why the lunar tide exceeds that raised by the sun (which, were the ratio that of the inverse square, would be the greater).—W. P. Thanks; we have touched on the point in our article on "Fallacies," now in type.—Edmund Hunt. Before we go to the expense of engraving your diagram, we must have some idea of the nature of the relations you propose to deal with. As it stands, it looks like "a beast." We give it without a figure, and in our own words, in the Mathematical Column; but we should not care to attack it unless we saw our way to some useful result.—H. A. Bulley. Let us rest, after magic squares, before we begin on magic circles.—W. R. Many thanks; your algebraical solution very neat.—A. N. Somerscales. Thanks; but the projection not likely to interest many, and space runs short. The polar aspect of the heavens appears.—A. T. C. When we wrote "any rectangle," we meant, as usual, that the solution must be applicable to any rectangle of whatever dimensions, not that you might take a rectangle of any particular dimensions which seemed most convenient. The question related to I.H.S., put on tombstones now, not to the ancient inscription in Greek letters, I.H.Σ.—C. J. C. May shortly describe a very simple instrument, by which the place of Venus in the day-time may be found.—E. D. G. Quite unable to find place for what you rightly describe as a flood of notes. Broun-Sequard was the name of the physiologist you refer to; but he did not write the article; he was quoted in it among other authorities. I wrote the article myself. The difficulty about recommending books is that the practice is open to abuse. — J. Whitley. Depends what size field you require. It is impossible to answer questions so vague.—R. Stavely. Thanks. But you get the wrong equation to escribed circle. The negative sign under the first radical is incorrect.—W. G. Rolfe. Astronomers are not at issue with geologists as to the earth's interior volcanic ashes carried by winds. In my article on earth-born meteors, I have shown how meteors sent beyond the earth's control would still travel on paths intersecting the earth's orbit.—A. M. K. T. You are quite right. I have written repeatedly to show that the only danger was for the comet, which, in such an encounter, would be like the "coo" of the elder Stephenson.—A. Jellivan. Never heard of any astronomer so named, but Prof. Pritchard may be writing about the stars.—R. E. J. That is just what I did take into account. If facing a windmill, the left arm *always* goes down, it follows that if the windmill faces the north, and you, facing it, look southwards, the eastern arm goes down. On the other hand, if you look northwards in facing it, the western arm does down. Why the left arm should be made to go down and the right arm up, instead of the reverse holding, I do not know.—Philo. Brackets are far too useful to be limited to the use to which Whately put them.—G. B. Thanks, but first set of hogs sufficiently attended to.—A. Gaubert. The Chinaman said he was a member of a class of trained computers, and not one of the most skilful.—G. M. T. Bates. It seems as though, since there are 462 combinations of six girls, and each set of five out of the six appears six times — viz., with each of the remaining six of the eleven, there must be one-sixth of 462 sets fulfilling the conditions, or seventy-seven.—G. M. If you wait till I prophesy, you will have to wait more than fifteen years.—Winter. The time of Venus's brilliancy is to be calculated, not taken from the "Nautical Almanack." You have not got the answer quite right to the watch question. The watch loses five minutes in a day of twenty-four hours, and the interval from noon at one place, to noon at a place 1¼° due west of it, is more than one day.—J.C.L., G.F.H., Geo. Brown, Araitchens, C.T.B, and others. Thanks for notes on the quicksilver matter.—Insomnolens. Your night-watching calculation of the sun's restraining force on earth, measured by tensions of sun-attached wires, is excellent. Wish we could find room for it. There is no doubt now on the general subject of light. Sound ceases to be perceptible when the vibrations are very rapid, and a very wide gulf separates the most rapidly vibrating sound-waves from the most slowly vibrating light-waves. The waves are also different in kind. To say truth, one might almost as well ask, Why, since water-waves rock ships, do not sound-waves also rock them? as your question, Why, since vibrations of a certain velocity produce sound, light-waves are not also heard?—Hesketh. A theodolite would not help you much to find a star or planet from the R.A. and Dec. in the *Nautical Almanac*. You can calculate by spherical trigonometry the altitude and azimuth for a given epoch; but a fresh calculation would be wanted after the epoch was past. The best way is to use an equatorially-mounted telescope. Of course, if the object is conspicuous, you can mark down its place in a star chart, and then readily identify it in the constellation wherein it is.—Scientiæ Amicus. Query why birds scratch their heads, rather too suggestive of Lord Dundreary's "Why doth a dog waggle hith tail?"—Churchman. That is just one of the questions we do not wish asked of the students of science to whom they refer. What they think of the origin of man is clear. If they think this inconsistent with other views, we do not wish them to say so; therefore, we do not wish others to ask them if they think so.—Jas. Grundy. When a person who has had small-pox, or has been vaccinated, is again attacked, it is not the revival of the former disease germs which is in question, but the introduction (in some way) of new germs into the system. Vaccination is supposed to act as a perfect protective for about seven years. Germs, however, may lie dormant for years, as we see in such cases of infection as are discussed in Tyndall's book on "Dust and Disease."—Ursa Major. The rule is—number of feet fallen in t seconds, equals $16{\cdot}1 \times$ square of t; so that in 6 seconds a body falls through 6 times 36, or 216 feet.—J. Sharp. Observations such as you cite are not of sufficient exactness to admit of scientific discussion. It is no proof of spontaneous generation to ask, If such and such living creatures were not evolved in the places where they are found, whence came they. In many cases, where the origin seemed more perplexing than in those you cite, Pasteur and others have traced the complete chain of linked existences.—Gravity. The difference in the range would not be so great as the parabolic theory would suggest, the resistance of the air greatly affecting the range; but there would be a decided difference of range. If both guns were fired at the same time, then, when the lower projectile had descended to the ground, the upper would have attained as great a horizontal distance, and be still 100 ft. from the ground; it would not be descending vertically, the range continuing to increase, though more and more slowly, till this projectile in turn reached the ground.—A. E. S. The question was answered on page facing the answer to other A. E. S.—J. A. S. B. Too big a question. All perturbations have to be considered, besides mathematical relations of a complex kind.—H. R. Weller. Thanks, but room for only one answer to the question.—C. E. H. By a *sine quâ non* is signified something without which a certain course will not be followed; the words mean "without which not."—I. J. Simpkin. Thanks for the corrections. The article was too technical for the "reader" and for our readers. It was sent to the printers by mistake for another.—Rev. W. J. W. Many thanks.—Constans. Nay, the poker, if—as usual—cold, in the first instance will do the reverse of what you suggest. If finally it gets warm, its warmth has been taken from the fire which it is supposed to nourish. In the other case, we know why less light reaches the eye in full sunlight. Stay in the dark awhile, with a small mirror in your hand, facing the blind which darkens the room, and while a friend draws up the blind, look at the pupils of your eyes as seen in the glass, and you will see the reason clearly enough. Can see no reason why with spring tides the sky should always become overcast; though, of course, there is then a wider water surface for evaporation.—John Sanders. The points of light are simply the images of Jupiter itself seen after double reflection at the front of the glass as well as the back. Children have been suffocated by cats sleeping over their face, not by cats drawing their breath. The secret of such criticism as that in the *Manchester Sporting Chronicle* is to be found in my condemnation of sporting rascaldom, not in my predictions of unpleasant things. It is very well known I have predicted only such things as meteoric showers, &c., which have usually occurred as predicted—unlike what sporting prophets predict; praise from sporting prophets would be like the contempt of honest folk.—J. P. Sandlands. Surely you are a little unreasonable. Certain readers ask for intermediate forms, saying that if there is descent, such forms should exist. Dr. Wilson describes some. You then say they do not prove descent; and I reply, naturally, that he wrote in response to those who thought such evidence as Dr. Wilson supplied essential to the development theory, and not for one who, like yourself, considered that it proved nothing. Now you say that Knowledge, being intended for those who want information, contributors should write for those who, like you, want information. Are we, then, to answer no questions until we are assured that every reader of Knowledge wants the information asked for by some of them? It would interest me to know how my article on "Fallacies about Luck" touched on religious questions. In one sense, everything almost that could be said here might be regarded as touching on religion. The statement that two and two make four, involving as it does the inference that two and one make three, might be regarded as verging on a reference to the diverse doctrines held by Trinitarians and Unitarians; remarks on Brain Troubles might be considered to refer, more or less directly, to theological interpretations of "possession by spirits;" Dr. Carpenter's discussion of Food questions might be held inconsistent with the Bible narrative of the basketsful of fish; and Mr. Foster's articles on Illusions might be

regarded as intended to explain away miracles. We expect fairer treatment than this, however, from our readers, whatever their religious views may be. The word "Simian," when referring to the species, always has a capital. As for "extraordinary," *does* it mean extra-ordinary in your sense—that is, exceptionally ordinary? Always thought it meant something outside of what is ordinary.—W. Greenwood. If the air in bladder is much compressed, there will be a slight excess of weight; otherwise none, unless weighed in vacuo.—F. Cowley. You are right in saying that we do not see a star where it actually is, if the star is in motion, though, owing to the much more rapid motion of light than of any celestial orb, we always see a star very near (apparently) to its true place. But you are quite mistaken in supposing that owing to the earth's rotation, the star's true place might be in the southern celestial hemisphere when we see the star in our northern skies. The earth's rotation has nothing to do with the matter—at least nothing in the way imagined. Compare the earth to a twirling globe in a room, against which a number of small shot are shot from a distance. The globe may have made several twirls while the shot were travelling, and the side turned towards the gun when it was fired may be on the opposite side when the shot arrives, but the shot will reach the side which is at the moment of arrival towards the gun; and, in like manner, rays from a star reach that side of earth which is towards the star, so that rays coming from a star really in the southern celestial hemisphere cannot possibly reach a part of the earth turned towards the northern celestial hemisphere. In other words, wherever a star's rays reach the earth (that is, wherever the observer may be), they will seem to come from the direction in which the star lies, apart from the slight corrections due to aberration, &c. The suggestion that expensive telescopes and microscopes might be let out on the three years' system seems worth considering.—F. F. Read article by "Fellow of Astronomical Society," in No. 10.—B. Riley. Your method of computing the moon's distance from the force of gravity, as calculated for the moon, combined with moon's known period, is simply working Newton's problem backwards. It is indicated at p. 21-2 of my Treatise on the Moon.—T. A. The lecturer was quite right, so far as astronomy teaches. I have for several years given a lecture bearing the same title and treating the subject in the same way. Matter may be infinite, but it does not follow that each sun in space can draw to himself an infinite quantity.—Vice-Admiral, F. A., &c. Thanks. The efficacy of oil as a sea-calmer has been warmly advocated by Mr. W. Chambers, in *Chambers' Journal*. Believe the matter still remains in doubt. Have not room for the long extracts sent both by F. A. and Vice-Admiral.—J. J. Your method already considered in the earlier numbers of Knowledge.—Farmer Will. Sorry to hear you say you have a "fossilised mind," but your belief in Noah is scarcely of scientific importance. Permit me to quote one sentence of your letter:—"Before Noah's time we read that there was a certain tree" the taste of whose fruit brought trouble. "In abusing knowledge we eat of that fruit." Will adverse critics, if such there are, remember?—J. H. H. I can answer from my own observation that all the objects you mention can be seen with the smaller apertures, powers, &c. Probably the faintness of green or blue stars may be due to the quality of your glass. The diffraction rings not being complete circles does not indicate very serious defect. Wish your letter were a little more compact, or your queries put separately, in proper query form.—B. M., F.R.C.S. You write under evident misapprehension as to space at our command. But thanks all the same. Surely the Osborne sea-serpent case did not occur so many as "eight or nine years" ago. Which questions would better suit the *Lancet*. We do not at all want medical questions; but unless you call the quicksilver question medical, none such have appeared.—Arthur Twort. Light from each point in the small triangular space gives circular image of sun, and these overlapping, combine into a single circular image, in which the triangularity of small aperture is lost.—A. J. Martin. It was a slip of the pen on the part of "Five of Clubs," which he corrected in the next number.—E. J. Wilson. Do not know any better work on trigonometry than Todhunter's.—J. A. Crawley. Thanks for quotation from Tacitus, showing that Tiberius was of the same mind as author of "Brain Troubles": "Solitusque eludere medicorum artes atque eos qui post tricesimum ætatis annum ad internoscenda corpori suo utilia vel noxia alieni consilii indigerent."—E. D. G. Thanks for numerous replies and notes. If Knowledge could but be expanded weekly to 48 pp. or so! Replies not mentioning number of query are useless to us.—J. H. Garfit. Fear I can only say the article was too long. When any circumstance assures us that an article will not suit, our attention is naturally turned at once to "the next article."—J. Rae. Whoever reaches the Pole will probably have to winter there. For him there are many ways.—J. J. Henderson wants titles of books on chemical analysis of alcoholic stimulants.—J. W. C. That would be right, if you have correctly determined focal length of the two glasses. But a 2-inch aperture would not readily bear such a power.—W. Baxter. Ferguson's tables no longer of any value. Try Johnson's book on eclipses.—Gradatim. The paradox comes in before the Pole is reached. The time is finite, but the number of convolutions infinite; how, then, can the particle be said to reach the Pole (along what course, I mean)? Thanks for other matter, but no space except for query.—R. W. Dr. Brewer's explanations amusing. "Why does sun put out fire? Because the chemical action of sun's rays is detrimental to combustion! Why does a poker across a fire revive it? Because the poker concentrates the heat!" Just so. This sort of science-teaching might go on for ever. As thus, why do the planets travel in ellipses? Because the tendencies of planetary motion are elliptical. Why do comets bring disaster to nations? Because of the disastrous tendencies of cometic apparitions.—A Student No. 1. You ask us to reconcile a statement made by Dr. Ball with another made by the Astronomer Royal for Ireland. Dr. Ball says in one lecture, there is no water in the moon; the Astronomer Royal for Ireland, in another, says tidal waves checked the moon's rotation. But Dr. Ball said in the latter lecture that such waves would exist whether there was water or not in the moon; and in the former lecture the Astronomer Royal for Ireland said that formerly there may have been water in the moon; while finally you are not to suppose that Dr. Ball says one thing as Dr. Ball and another as Astronomer Royal for Ireland. This you will have opportunity of recognising shortly in an article which he has written at our request for Knowledge.—A Student No. 2. Stimulants certainly not good; the other matters depend on the health, strength, and constitution of the student.—B. Your vernier reads more closely by being so divided; adding the extra divisions is equivalent to bisecting the divisions on the limb.—G. G. D. Measured from true noon, the change is equal on either side, but mean noon slightly differs from true noon. Read any text-book account of equation of time. Fully answered in ordinary explanations of the calendar.—F. H. R. Edinburgh is so often pronounced Edinburg, that custom may be regarded as at least justifying the practice. In Scotland I have seldom heard it, but always Edinbro', among the less educated often Embro'.—W. Wilson. Cannot see how a scientific theory can depend on the merely verbal questions you raise. I would rather, for my own part, say, "I see the house," than, as you suggest, "I see the vibrating ether; or, I have received on my consciousness, through the retina, the optic nerves, and the brain, an impression by vibrating ether," indicating the existence of a house, &c.—Gerald Massey.—The motion of the apsides does not affect the precessional period. It shortens the interval between the epochs when spring, or any other fixed seasonal point, coincides with perihelion or aphelion, but the spring equinox makes the circuit of the ecliptic in the period of about 25,870 years mentioned in books on astronomy.—J. A. Dobson mentions that Drew's Geometrical Conics is sufficient for a first class in fourth stage at Kensington.—M. H. P. The dream theory of your lecturer has no scientific basis.

Letters Received.

T. V. H., A. Aitken, Acacy, S. de M., J. B. Dimbleby, E. M., J. C. H., F. W. Bockett, J. R. H., J. J. M., Isaac Isaacs, Cosmos, Charles Gray, X., Amicus, G. A. L., J. A. Miles, Magic Squares, Student, (not A Student), Vega.

Notes on Art and Science.

An Electrical Stature Alarm.—A curious application of electricity is described in *La Lumière Électrique*. It consists in a device to prevent military conscripts practising frauds as to their stature by bending their knees. When the youth stands erect against the measuring post, the hind parts of the knees press on electric contacts, causing two bells to ring; the ringing ceases when there is the least bending. The sliding bar which furnishes the measure has also a contact, which is pressed by the head, whereby a third electric bell is affected. For a correct measurement, the three bells should ring simultaneously. This system, the invention of M. Cazala, is now employed in the Spanish army.

Capacities of Lungs.—Dr. Nagorsky, having measured the capacities of lungs of 630 boys and 314 girls in the schools of the district of St. Petersburg, now publishes the results of his investigation in a Russian medical paper, the *Surgeon*. He has found that the capacity of lungs, in relation to the weight of the body, is 65 cubic centimètres for each kilogramme of weight in boys, and 57 cubic centimètres for girls. The law of Quetelet being that, with children below fifteen years of age, the weight of the body is proportionate to the square of the height, Dr. Nagorsky has

XUM

found that it is proportional to 2·15 of the same; while the capacity of lungs is proportional to 2·4 of the height for boys, and to the square of the height for girls. Dr. Nagorsky's researches will soon be published in a separate work. As to the relation between the weight of man and the capacity of lungs, it is tolerably permanent, and its variations are mostly due to differences in the amount of fat in the bodies of different men.

Explosion of Aqua Ammoniæ.—The *Pharmaceutical Journal* records a recent case of an explosion of ordinary liquor ammoniæ, followed by serious results. A Belfast woman, subject to headache, sent her daughter to the druggist to purchase a small quantity of "head salts," for which he gave her liquor ammoniæ, or "spirit of hartshorn," instead of the salt, carbonate of ammonia. The vial was put on a shelf and not used for a few days. Having a headache, the woman lifted the remedy to apply it, and had it in her hand for a few minutes only when the vial suddenly exploded, scattering the contents over her face. Her eye was destroyed, and her mouth and throat burned, the skin of both having been torn off. The vial had been put on the mantelpiece previous to the time it was used, and when about to apply the contents the woman was sitting near the fire.

A new work, by Mr. Richard Meade, Assistant Keeper of Mining Records, entitled "The Coal and Iron Industries of the United Kingdom," will be issued about the 15th inst., by Messrs. Crosby, Lockwood, & Co. Besides a description of the coal-fields and the principal seams of coal, Mr. Meade's book will include an account of the occurrence of iron ores in veins and seams, and a history of the rise and progress of pig-iron manufacture since the year 1740. Maps illustrating the position of coal-fields and iron-stone deposits throughout the kingdom will accompany the work. Messrs. Crosby, Lockwood, & Co., will also issue, during the month of February, a new work, by Mr. Lowis D'A. Jackson, author of "Hydraulic Manual and Statistics," entitled "Modern Metrology." This manual will treat of the metrical units and systems of the present century, and will include an appendix containing a proposed English system. The book will, we understand, be dedicated, by permission, to the Right Hon. W. E. Gladstone.

Our Mathematical Column.

MATHEMATICAL QUERIES.

[29]—An ellipse has semi-diameters DB, DF ($DB=2DF$). From D, DC is drawn, making an angle $\cot^{-1}2$ with DB (on the same side as DF), and DA making angle ADC = angle CDB. Make $DA=DB$, and draw ACB. From AD cut off AE = one-fourth AD. With centre E describe circle AGH, cutting elliptic quadrant AF in H and G (G nearer to F). Draw GI perp. to ED. Let AI cut DC in J. With centre B and radius CJ describe circle cutting CD in K, and with centre C describe circular arc KL, cutting KB in L. It is required to determine geometrically the ratio of the arc KL to the straight line CB.—Edmund Hunt.

[30]—Simultaneous Equations.—

$$x^2 + y = 11$$
$$y^2 + x = 7.$$

—Thomas Fawcett.

[This equation can readily be solved, so far as finding the obvious roots is concerned; on the other hand, if a and b be written for 11 and 7, the equation cannot be reduced to a quadratic. We leave the equation in the above form, as an exercise for the young reader.—Ed.]

[31]—Equation.—

$$x^4 + 4x^3 = 27.$$

—W. H. B.

[32]—How to Analyse a Curve?—A curved object has to be reproduced in different sizes. A tracing of its curve has been made on paper. How can an analysis and definition of the curve most easily be arrived at? A base line has been drawn on the convex side of the curve and offsets taken to the curve as noted below:—

Base line	0	·15	·25	·35	·6	·7	·85
Offsets	2·3	1·7	1·5	1·3	1·	·9	·8
Base line	1·0	1·2	1·45	1·75	1·95	2·1	
Offsets	·7	·6	·5	·4	·35	·35	
Base line	2.4	2·65	3·0	3·5	4·1		
Offsets	2·25	·2	·15	·1	·05		

The measurements being in inches and decimals, can the law or laws of the course of the curve be deduced from these figures? If yes, how?—Fred. W. Foster.

[33]—Bisect a triangle by a line drawn from a given point outside the triangle.—J. A. Dodson.

[34]—Eleven school-girls went for a walk every day, and were each offered a bouquet every day so long as a different six presented themselves to the giver, and provided also that no *five* girls ever found themselves in the same group twice. On how many days could they so arrange themselves?—G. M. T. Bates.

[22]—The equations are—

$$(\alpha)\quad \frac{x^2}{a}+\frac{y^2}{b}=\frac{a^2}{x}+\frac{b^2}{y}=a+b$$

($x=a$, $y=b$, obviously simult. roots.)

$$(\beta)\quad 2x\sqrt{x^2+a^2}+2x\sqrt{x^2+b^2}=a^2-b^2$$

May I suggest another astronomical problem? A lunation is 29·53059 days, and period of the sideral revolution of the moon's node is 6799·5 days; show that after 14558 days eclipses may be expected to recur in an invariable order.—Rueverte.

[22*a*]—Correcting the obvious misprints, we have

$$\frac{x^2}{a}+\frac{y^2}{b}=a+b \quad \ldots\ldots \text{(i)}$$

$$\frac{a^2}{x}+\frac{b^2}{y}=a+b \quad \ldots\ldots \text{(ii)}$$

One solution is obviously $x=a$, $y=b$. But proceeding *secundum artem*, *i.e.*, finding value of x from (ii), substituting in (i), and reducing, we get

$$(a+b)y^4-2b^2y^3-3ab^2y^2+2b^3(a+b)y-b^5=0$$

As $y=b$ is a known root, it follows that $y-b$ is a factor of this equation. Dividing, then, by $y-b$,

$$(a+b)\ y^3+(ab-b^2)\ y^2-(2ab^2+b^3)\ y+b^4=0.$$

On trial, $y-b$ is found to be a factor of the new equation, and we get $(a+b)\ y^2+2aby-b^3=0$, a quadratic,

whence $$y=\frac{b}{a+b}(-a\pm\sqrt{a^2+ab+b^2}),$$

besides the previously obtained values b, and b.

Then, by symmetry, the values of x are

$$a, \text{ and } a, \text{ and } \frac{a}{a+b}(-b\mp\sqrt{a^2+ab+b^2})$$

F. J. Butt.

I should be glad of a better solution of the following than I have been able to devise.

$$x^2=\frac{39}{y}-\frac{14}{x}$$
$$y^2=\frac{42}{x}-\frac{13}{y}$$

Science and Art Department, May, 1881.

In my hands it becomes, by substitution, from (i) in (ii) $x^9-84x^6+1623x^3-21952=0$, which, treated as a cubic, and solved by Cardan's rule, gives $x=4$, and $y=2$, besides imaginary values. But I think there must be a shorter cut.—F. J. Butt.

$[x^2+3y^2=\frac{112}{x}$; $3x^2+y^2=\frac{104}{y}$; whence $(x+y)^3=216$, $x+y=6$.

Then $3\,(6-y)^2+y^2=\frac{104}{y}$; $3(6-y)^2y+y^3=104$, giving $(y-3)^3=-1$; $y=2$; $x=4$, with imaginary roots.—Ed.]

[28]

$$\frac{a+x}{\sqrt{a}+\sqrt{a+x}}+\frac{a+x}{\sqrt{a}-\sqrt{a-x}}=\sqrt{a}$$

rationalise denom., and we get

$$(a+x)^{\frac{3}{2}}+(a-x)^{\frac{3}{2}}=3x\sqrt{a}$$

square $\quad (a+x)^3+(a-x)^3+2\,(a^2-x^2)^{\frac{3}{2}}=9\,a\,x^2$

i.e. $\quad 2\,a^3-3\,ax^2=-2\,(a^2-x^2)^{\frac{3}{2}}$

square $\quad 4\,a^6+9\,a^2x^4-12\,a^4x^2$
$$=4a^6-12a^4x^2+12a^2x^4-4x^6$$

i.e. $\quad 4x^6=3a^2x^4$, from which we get

$$x=0, \text{ or } \pm\sqrt{\frac{3}{2}}a$$

W. N. W.

[Similarly solved by Yarletonian and others.]

Our Whist Column.

THE GAME IN No. XIII.

Your correspondent, "Five of Clubs," in his observations on *Z*'s play (p. 284), approves of *Z*'s holding up the turn-up card, and playing a higher one to the adversary's lead of trumps because he thereby gives information to his partner. Now I submit that on an adverse lead of trumps, the right rule is to give the ad-

versaries as little information as possible as to the number one holds, and for that reason one ought, as a rule, to play the turn-up card, when a small one, as soon as possible. Giving information in the case of an adverse lead of trumps is so, it seems to me, sacrificing the real object in play, viz., trick getting to the principle of giving information, which is only a means to an end. But this is a result to which the teaching of those who make this principle their hobby, and assert it to be the basis of modern scientific Whist, necessarily leads. The principle of giving information of strength to a partner is as old as the game itself, and is sound, because, as a rule, it is information by which a partner can gain more advantage than the adversaries, but I am sure that to tell the table of one's weakness is giving information of which the adversaries can avail themselves more to one's detriment, than a partner can to one's advantage. If anyone doubts it, let him play dummy. If dummy's partner, he will find how much less use it is to him to know dummy's cards when dummy is weak, than when he is strong. If dummy's adversary, he will, on the contrary, find that he can take much more advantage of knowing dummy's weakness than he can of his strength. So that, judging by experience and by reasoning, I believe that the old rule of trying to conceal weakness, even at the risk of deceiving your partner as well as your adversary, is sound. I dwell upon this point, because it is one on which I consider "Five of Clubs" essentially wrong. He, at page 42, says in effect that there is *no limit* to the principle "that it is more important to inform your partner than to deceive your adversary." If this be true, *A* and *B* playing against *Y* and *Z* would gain an advantage by being allowed to expose their hands on the table, whilst *Y* and *Z* held up theirs. But does any reasonable man believe this? I suppose not, but if not, there is some limit. Let us apply a further test. Suppose *A* is permitted to expose his entire hand whenever he chooses, but has to play it himself, does "Five of Clubs" really contend that *A* would be wise to do so when he has a weak hand? If he does not, he admits a still further limit to what he calls the great principle of the modern scientific game, and really comes back to the principles taught by Hoyle and Mathews. I am aware that "Five of Clubs" says that these two are out of date, but I am not alone in the opinion that Mathews' treatise is the one of all others most likely to develope the powers of a player, to make him use the rules as his servants, instead of being their servant, and to enable him to cross the line which divides the mere book player from the first-class player.

One word more, and I have done. Will "Five of Clubs" refer to his observation 9, p. 284, and explain why *Z*, after two rounds of clubs, will, because *Y* plays the ten, be able to place the Knave of Clubs in *Y*'s hand? *Y*'s play of the Club ten strikes me as decidedly bad; he was, of course, bound to keep up the small Club, but as it could not matter which of the three higher ones he played if *Z* held the Queen of Clubs, as well as the Ace and King, he was bound to play on the assumption that *Z* held only the Ace and King; now, by playing the ten, *Y* would tell *Z* that the nine was in the hands of the adversaries, and *Z* would properly conclude that if either adversary held the Queen or Knave with the nine, he, *Z*, could not get the command of the suit by leading the Ace, and would, therefore, feel bound to lead a small one upon the chance of *Y* holding the Queen, the very thing which *Y*, having four clubs, ought to try to prevent *Z* doing.—MOGUL.

[I am obliged to MOGUL for pointing out what he considers to be faults in my views respecting Whist strategy, because in this, as in all other matters depending on experience and reasoning combined, the truth can only be got at by what may be described as a system of trial and error. With regard to "Mogul's" general view on the question, whether is is better or not to give as much information to partner as can be given consistently with the rules of the game, I may for the present content myself with the remark that modern Whist adopts the principle which he considers unsound, and that, this being so, one can hardly depart from it without actually deceiving partner (which is not quite the same thing as failing to inform him). Clay has clearly shown in his chapter on False Cards, that the result of attempting to keep the adversaries in ignorance of the constitution of one's hand may in this way be most mischievous. It may be the case that in a particular hand, giving information of weakness may do more harm than can be compensated by any advantage; but, in the long run, uniform play in each hand (up to the point when the strategy of the hand has been fully developed) is best, the play of false cards in one hand (though, perhaps, saving from immediate disaster) leading to doubt and mistrust in many others.

In passing, I may note that playing dummy (single dummy, presumably, is meant) is not a trustworthy test of the system of modern Whist; for, in a number of cases, what may be information to one's partner in the usual game may be no information at all to the adversaries.

If anything I said at page 42 means in effect that *there is no limit* to the principle "that it is more important to inform your partner than to deceive your adversary," then I certainly said what was entirely and egregiously wrong. It is often absolutely essential to success in the closing rounds of a hand to deceive the adversary, whether partner be at the same time deceived or not.

Turning now to the game at p. 284, I note first that I entirely differ from "Mogul" as to *Z*'s policy in playing five of trumps and holding up four (the turn-up card). I had not expressed approval—simply noted why *Z* had done this. But the reason which "Mogul" urges against *Z*'s play does not exist. The adverse lead of trumps is not such as to indicate more strength than *Z* possesses himself. *A* has led trumps when his own suit is exhausted from one of his adversaries' hands as well as from his partner's. *Z* can be certain that *A* has not more than four trumps one honour, or three trumps two honours, for if he had had more he would have led trumps earlier, with such strength as he had (and has already shown) in spades and diamonds. Now *Z* has himself four trumps, headed by Ace, ten. He has a long suit headed by Ace, King, Queen. His only chance is in playing as with strength in trumps; and his policy is therefore to show his partner all he can of his trump suit. The game is gone anyway if *Z*'s strength in trumps shall prove insufficient to bring in the long suit, and he simply plays as if he knew for certain that he had sufficient strength. If the score were "love all," the case would be different; *Z*'s policy would then have been to play a backward game, so as to lose as few by tricks as possible. But playing a backward game as the score stood would have been the same thing as throwing up the cards.

Similar remarks apply to *Y*'s play at trick 9, which was rather warmly canvassed after the evening's play. Consider what *Y* knows, what *Z* knows, and (which is quite another question, and very often overlooked) what *Y* knows that *Z* knows. *Y* knows that the Diamond King cannot be with *Z*, or *Z* would have played it as soon as trumps were out. He knows that *Z* cannot credit *Y* wrongly with the Diamond King, for at trick 2 *Y* would have put it on *A*'s Queen, if he had had it. *Z* knows his partner has no more spades (this is clear from *A*'s play in leading trumps at trick 5, which he would not have done if there were two Spades outside his own hand—knowing his partner with none). *Z*, then, can put three Spades in *A*'s hands, three Diamonds at least (from penultimate lead, and four if he has noticed that the lead was really from the ante-penultimate) in *B*'s hand, headed by Diamond King. Also from *B*'s discard of Club five, when only one trick was wanting to win, *Y* knows that if *Z* has not the Queen himself he will certainly not place it in *B*'s hand. Thus *Y* knows that if *Z* has not Ace, King, and three Clubs, he would throw up the cards, for in that case one trick *must* go to *AB*. He knows that *Z* is certain to draw the Queen if it lies with *A*. And he sees that among the various cards, including Queen, with which *Z might* credit *Y*, there is only one which would justify the lead of a small card; viz., if *Y* held ten, Knave, Queen, and no other Club. But as *Z* ought to know *A* with only two Clubs, and *B* with only one (it is not *Y*'s fault if *Z* has not noticed *B*'s ante-penultimate lead—tricks 2 and 3), *Y* must have four Clubs. By playing as he does, then *Y* does, not run the risk Mogul suggests; while if *Z* has Ace, King, and draws the Queen from *A* (who, so far as *Y* knows, may hold it), *Z* will know from *Y*'s play in the second round of Clubs that Knave must lie with him, his attention being in the most marked manner called to this by the unusual play of 10 before 9. As I said in the notes, this was not essential to the success of *Z Y*, for if *Z* played according to the fall of the cards, he could not fail to draw *Y*'s Knave with a small card (if not holding Queen), but it was well to call his attention to the point. Note that if *Z* had been inattentive to the earlier play, and supposed *Y* to have held originally either Queen, Knave, ten, and a small one, or Queen, ten, and a small one (we are considering the matter from *Y* point of view, who does not know that *Z* holds the Queen), it would be unnecessary to lead a small one, for *Y*'s small one could be used, after Queen and Knave were played in one case, or Queen in the other, to put the lead again in *Z*'s hands. If *Z* thought that *Y* held Queen and ten only, originally, playing a small one would be fatal, as, after winning with Queen, *Y* would have no Club left. With only one hand out of four possible ones, would lead of small one be right—viz., if *Y* had Queen, Knave, ten, in which case if *Z* played the Ace before the small one, *Y* winning third trick in Clubs, would have no Club to return with. Only one chance in four suggesting lead of small one, *Z* would, of course, play the King. As a matter of fact, he ought to know that *Y* could not hold that particular hand. I am disposed to think *Y* displayed unnecessary ingenuity—which, as I chanced to be his partner—was perhaps, natural, for partners seldom criticise without prejudice.

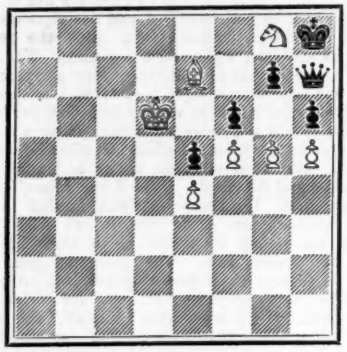
FIVE OF CLUBS.

Our Chess Column.

No. 17.

End game study, by the veteran master, Herr B. Horwitz.

Black.



White.

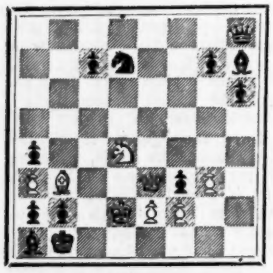
White to play and win.

Mr. Grimshaw has amended his problem in a very ingenious manner, adopting the position by the addition of simply a Pawn to both the solutions that his original problem admitted. He has thereby made two problems out of one, which, although looking almost exactly alike, nevertheless embody different ideas.

No. 18.

By W. Grimshaw.

Black.



White.

White to play and mate in three moves.

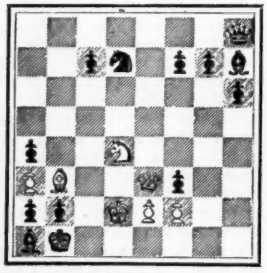
Solution.

Kt. to B.5. B. takes Kt.
Q. to K.6. and mates next move.

No. 19.

By W. Grimshaw.

Black.



White.

White to play and mate in three moves.

Solution.

Q. to Kt.5. P. takes Q.
Kt. to Kt.5. and mates next move.

It will be seen that the addition of a White Pawn in the first problem on Kt.3 renders Q. to Kt. 5 useless, as White now does not threaten mate by Q. from Kt.5 to Kt.sq. Then again, in the second problem, the addition of a Black Pawn on B.2 prevents the first solution, as, after Kt. to B.5, B. takes Kt., the Q., of course, cannot now go to K.6. We are glad to see that our remarks on the original problem, published in the *Illustrated London News*, have had such a satisfactory result. The twin-problem, as it may be called, is a perfect Chess curiosity.

CORRECTION.

Solution of Problem in No. 8, page 171.

White.	Black.
1. K. to K.3.	1. K. takes Kt.
2. R. to Q.Kt.7.	2. K. takes R.
3. R. to K.7, mate.	

or,

	2. Any other move.
3. Rook discovers check accordingly, and mates,	

or,

	1. B. takes P.(ch.)
2. R. to Q.4., dis. ch.	2. K. to K.3.
3. R. to B.6. mate.	

or,

	1. B. to B.5.(ch.)
2. Q.R. takes B., dis. ch.	2. K. to K.3.
3. Q.R. to B.6. mate.	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

W. Godden.—Solution of Problem No. 14 correct. You are right; it is a beautiful composition.

A. J. Martin.—If you assert anything most emphatically, and especially if you are right, as you were in this case, you need never fear nor tremble! See correction. Self-mate is where White compels Black to mate him.

J. P.—You were right; see correction. No. 14 right.

B. Pierce.—In the Evans Gambit, against correct defence the first player loses less than a Pawn, as he has some attack for the abandoned Pawn. You can safely decline the Gambit by playing 4. B. to Q.Kt.3. We, however, prefer to accept it.

G. Woodcock.—No. 9, incorrect—P. to B.6. is the move; 10, correct; 11, incorrect; 12, correct. In note (a), read P. to K.R.4.; in note (h), R. to B.sq.

Mahnie.—No. 14, incorrect—B. to R.4. is the move; 15, correct.

A. C. Skinner.—Solution of No. 14 correct and neat.

Vicar.—Solution of No. 15, correct; 14, B. to R.4.

W. Thurman.—Solutions correct. Thanks for problem.

Salford.—Solution of No. 11 incorrect.

S. L. P.—Solution of No. 14 correct, only you have reversed the board.

Geo. O'Donnell.—Solution of No. 14 incorrect—try B. to R.4. In No. 15, White compels Black to mate him in two moves.

G. M.—Solution of No. 14 incorrect—try B. to R.4.

Arthur Black.—Received with thanks.

F. H. I.—Solutions correct.

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